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PHYSIOLOGY.

LECTURES
ON THE
PHYSICAL CONDITIONS
OF THE
TISSUES OF THE HUMAN BODY,
AS APPLIED TO THE EXPLANATION OF
VITAL PHENOMENA.

Delivered at the College of France in 1834.

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LECTURE XVIII.

Deductions in Support of the Lecturer's Theory of the Sounds of the Heart, derived from Auscultation during Pregnancy, and from Cases of Anomalous Disposition, and Partial Solidity, of the Lung, and Diseases of the Heart and Chest; also from Experiments, on the Heart, which prevent its Shock against the Ribs, and from Impediments to a free and quick Ventricular Entrance and Expulsion of the Blood.—Physical Circumstances which account for the particular Characters of the two Normal Bruits of the Heart.—Illustration from Laennec.

GENTLEMEN,—At the close of my last lecture I asserted that the bruit of the heart does not reside in the organ itself, that is to say, is not produced by any mechanism in the interior of the heart, or by a concurrence of circumstances independent of the surrounding organs. I showed you this clearly in the heart of the swan, whose sternum we removed. Upon opening the pericardium and placing the ear close to the heart, or even employing the stethoscope, no bruit or sound of any kind was to be distinguished. This, I should think, is a sufficient answer to those who refer the sounds of the heart to the *frottement*, or the impulsion of the blood against its parietes, valves, &c. Besides this, we may bring forward another argument, drawn from what is observed in the circulation of the child during pregnancy. You all know that auscultation has of latter years been much employed as a means of determining in doubtful cases, whether a woman be pregnant or not; when the stethoscope is placed on the side of the abdomen, you are frequently enabled to distinguish the pulsations of the heart of the child. Now if the sound in this case proceeded simply from some action taking place in the interior of the heart itself, there is no reason why we should not distinguish it at all periods of pregnancy; but this we cannot do. All accoucheurs are agreed upon the impossibility of recognising the pulsations of the fœtal heart before the fifth, or, at least, the fourth month of gestation, after which time they are, commonly, well distinguished. Now this fact proves two things; 1st. That in the latter months the sound is transmitted with a sufficient degree of clearness through the medium of a fluid, the liquor amnii, and through the soft parts of the abdomen, to the ear of the auscultator; and, 2ndly, That the bruit of the heart is not produced by the shock of the blood against the auriculo-ventricular aortic valves, because, were this the case, we should hear the shock at the second or third month, whereas it is never with certainty heard before the fifth or sixth month, the period at which the thorax of the child begins to acquire some solidity. Remember that I do not speak here of the placental bruit, but of the sound resulting from the action of the heart; and permit me to call your attention to the manner in which this circumstance illustrates and is explained by our theory. Before the fourth or fifth month of fœtal life, you are unable to distinguish anything like the pulsations of the heart, because, previous to that period, the occurrence of a shock is impossible, from the want of development of the child's thorax.

In one of my former lectures I also observed to you, that whatever prevented

the heart from striking against the parietes of the chest, equally prevented the production of its sound. Let us now follow up this proposition in a little detail, as it is a natural and necessary deduction from the theory which I have had the honour of laying before you.

Laennec, Gentlemen, was a great and an acute observer of disease. As a clinical physician, there are very few who have surpassed him, while his physiological deductions from the symptoms before him were in most cases drawn with the utmost rigour. He observed that occasionally the bruit of the heart was very dull (although the contractions continued strong), or even sometimes altogether imperceptible, and that this depended generally upon an anomalous disposition of the lung, in virtue of which, the pleuræ and anterior borders of the lungs extend in front of the heart. This is a case which may often fall under your notice in practice. You place your ear to a patient's chest, over the region of the heart, and instead of hearing its pulsation, as you expected, you find a very peculiar sort of bruit, produced by the manner in which the contractions of the ventricles press out the air in that portion of the lung which is placed between the heart and the sternum: the sound of the heart is, perhaps, completely masked. You seek for an explanation of this extraordinary circumstance, and find it thus;—a soft body is placed between the heart and the parietes of the thorax; this prevents any shock, and, of consequence, the production of any sound. This, Gentlemen, is, as I said, a new proof of our theory; for if the sounds of the heart were produced within the organ, we should have no difficulty of hearing them through the portion of lung, in the same way as we distinguish the bruit arising from diseased valves, through the lung; hence, whenever the normal bruit of the heart is not to be heard at the usual point of the thorax, while its place is supplied by a peculiar sound, called the "bruit de soufflet," you may be sure that these phenomena depend on the interposition of a portion of lung, between the point of the heart and the anterior wall of the thorax.

This, Gentlemen, is not a mere conjecture: Laennec has observed the anomalous disposition of which I speak, and its effects in masking the true sound of the heart; and I have often observed the same circumstance myself, during the autopsies which are made at the *Hotel Dieu*. But a portion of lung may be interposed between the heart and the thorax, and yet the sounds of the former organ be distinctly heard. How do we explain this, what, at first sight, would seem to contradict the consequences we have drawn from our first proposition? In these cases, Gentlemen, there is, indeed, a foreign body interposed between the heart and the thorax, but it is no longer a soft one; the portion of lung has been rendered solid by the effect of previous disease, by inflammation, by hepatization, by the deposit of tubercles or other hard matter in its cellular tissue, and hence has become proper for the transmission of sound, or, rather, the heart strikes against the solidified mass with the same effect as if it struck against the thorax itself. So much for those cases in which the first sound of the heart sometimes disappears, and for the rational explanation they receive from the theory of the shock.

Again, if the heart be very voluminous, if the substance of the ventricles be extremely hypertrophied, the sound of the heart disappears just as completely as if a soft body were interposed between it and the chest. You may find examples of this affection every day in the hospitals, and on examining the patient, although the heart continues to beat with the greatest vigour, you do not distinguish any sound. Now here is a phenomenon which cannot be explained on any other theory as yet proposed by physiologists, and yet receives a most natural and simple solution from ours. To have a sound, we must have a shock, and a shock requires a certain quantity of room in the chest; the heart must not be too closely applied against the parietes of the thorax, and must have the power of advancing and receding to a certain extent; but in these cases there is no movement of translation; the heart does not retire from the sternum, and cannot, therefore, produce any shock. If you watch the motions of the chest, you see it violently lifted up by the expansion of the heart, but when you auscult, you hear no bruit whatever. This, Gentlemen, is in strict conformity with the physical laws of sound, which can never be developed by mere pressure, however strongly applied; a shock of one body against another is requisite, and where abnormal circumstances intervene to prevent this shock, you will invariably find that one or both bruits of the heart have disappeared according to the nature of the impediment.

I shall give you a short account of a case which occurred some time ago at the *Hotel Dieu*, and which is the more valuable, as the precise nature and seat of the affection was ascertained after the patient's death. The consequences we may draw from it are, therefore, not speculative theories, but merely the expression of facts. A female, who had come from the

country to Paris to be treated for an affection of the heart, entered the Hotel Dieu in the month of January last. She was young, and of a strong and vigorous constitution, but the disease had been permitted to go on so long a time without any treatment being applied, that on her entry into the medical wards of the hospital, it was a difficult matter to give a precise or correct diagnosis of her malady. This, Gentlemen, is a circumwhich very often happens. At the commencement, the symptoms of diseased heart are not only few, but very obscure; a patient may have some pain in the chest, about the region of the heart, some impediment to free respiration, and some palpitation, with irregularities in the pulse, and these symptoms, occasionally aggravated, may go on for a long time before the signs characteristic of the peculiar alteration which affects a portion of the heart manifest themselves; and even then, when the disease is far advanced, it requires a perfect knowledge of the art, and long experience, not to be deceived as to the nature of the affection, where rational signs are either confounded together, or are discriminated with great difficulty. Thus in the case of which I now speak to you, amongst other symptoms, the first sound of the heart was heard audibly enough, but the second bruit could not be distinguished, although the precordial region was examined with the greatest attention. What could be the cause of this curious and novel phenomenon? I confess to you that at the time I was totally unable to resolve the difficulty. Excessive hypertrophy, effusion into the chest, or any other mechanical impediment to the heart's shock which we are accustomed to find on examining the dead body, would equally have prevented the first bruit from being heard. However, in a few days the woman died very suddenly, and the examination of the body afforded us an opportunity of ascertaining the pathological cause of a symptom which we had not witnessed. The left side of the heart was hypertrophied, though not very extensively, but the pericardium and membrane lining the anterior surface of the heart were the seat of a most intense inflammation. Here is the pathological piece which I had taken from the body, and prepared in alcohol. I consider it as a most valuable preparation, not only as a fine specimen of pericarditis, but above all as illustrating and giving a satisfactory explanation of a physical symptom, which during the patient's life gave us no small embarrassment. You perceive how the anterior surface of the heart, particularly at the upper part, is lined by a false membrane, the product of inflammation which appears to have been of some date. The apex of the organ, as you may see, was comparatively free from disease, and was not confined in its motions; it had room enough to shock against the chest, and thus produce the first bruit, which, as I told you, was distinct enough, but the whole of the superior moiety of the heart was covered with a thick organized membrane, which you see here, in some places, at least one inch in depth. This adventitious deposit formed a bond of union connecting it with the pericardium, which itself was closely united by a strong membrane to the posterior surface of the sternum. Here, then, we have a sufficient explanation of the absence of the second bruit; the coagulable lymph which connected the heart to the pericardium, and this latter to the sternum, prevented the organ from retiring into the chest, and striking against the sternum, on the dilatation of the ventricles. There was no sound, because a shock was physically impossible. Thus, Gentlemen, you see how the theory of the shock not only explains the phenomena taking place in the normal state of the circulation, but also certain pathological conditions, which without it would remain lost to the science, or be completely unintelligible to the physiological observer.

Let us now turn to another case, nearly of the same kind: there is no difficulty in quoting cases, for when once we have succeeded in discovering a fact, it is generally easy enough to find its application. Not long ago a woman was received into the Salle Sainte Monique at the *Hotel Dieu*: although she was suffering from a disease essentially differing from that of our first patient, yet she exhibited this symptom in common, that the first sound of the heart was audible, while the second could not be discovered even by the stethoscope. In this case, however, the rational symptoms were sufficiently well marked to betray the proximate cause of her disease. On percussing the anterior walls of the chest, we found that effusion to some extent had taken place into the right cavity, and, instructed by the autopsy of the former patient, we concluded that the fluid passed upwards behind the heart, and thus prevented the organ from retiring so as to strike with any force against the sternum. This conjecture, Gentlemen, was found to be correct on the examination of our patient, whose death soon took place; but we also discovered a circumstance which we could not have foreseen, although it confirms, in a most decided manner, our theory of the sounds of the heart. A peculiar pouch of fluid, something like an hydatid, extended ob-

liquely upwards, insinuated itself between the vertebral column and the heart, and thus pressed that organ forcibly against the posterior surface of the sternum. The absence of the second bruit was, therefore, to be accounted for in the same way as in the case of the patient whose heart adhered to the sternum, although the cause of this absence was totally different, and in no way connected with disease of the circulating system.

The influence exercised by mechanical impediments of this kind in modifying or suppressing the sounds of the heart, may be easily demonstrated by experiments on living animals. Thus the chest of a dog, or other quadruped may be opened, and having insinuated the finger behind the heart, that organ may be pressed gently forwards, but so as to keep it in constant apposition with the parietes of the thorax. If proper care be taken to prevent the heart from receding, no bruit whatever is produced, although it continues to contract with increased force and quickness. This experiment, Gentlemen, is one of a most decisive nature; for why should the sounds of the heart be thus suddenly interrupted by a manœuvre which does no injury to the organ, unless we allow that when the shock is interrupted the bruit ceases? And here above all others you cannot attribute the normal sound to any action of the blood on the valves of the heart, for as the latter is pressed closely against the sternum, if this supposed valvular bruit did exist, it should be heard during the experiment we have mentioned, but no such sound nor any other is audible. Instead of employing your finger, you may inject a large quantity of air, water, or other fluid, into the cavity of the chest, and give rise precisely to the same result as before. Hence we may lay it down as a general rule, "that whenever an impediment exists of a nature sufficient to prevent the heart from shocking against the parietes of the thorax, the normal sounds of the heart will cease to be heard."

Besides the cases we have mentioned, there are other pathological circumstances also which prevent the heart's shock, and, consequently, the development of any bruit. We shall now mention some of these, as they contribute to support the theory we have advanced. In a former lecture I showed you what circumstances of position, space in the chest, &c. were necessary to permit the heart to strike against the thoracic parietes; but there are others, which may now be briefly noticed. Thus, in order to shock against the anterior wall of the chest, the heart must not only have room to retire, but also to contract with a certain degree of energy and force; it must expel the blood quickly from its cavities, for without this latter circumstance, especially, the heart cannot move suddenly forward at the time of its systole. If then the blood contained in the ventricles is not freely and quickly expelled when they contract, the shock of the heart's point becomes proportionately feeble, and the first is either very indistinct, or, in some cases, disappears, and is replaced by one totally different.

I had a case lately at the hospital, which has suggested these reflections:—A patient affected with disease of the heart was taken in, and on examining her chest we found that the first sound of the heart had quite disappeared, but instead of it we heard that peculiar bruit which indicates an alteration in the diameter of the ventriculo-aortic opening; the state of the pulse showed what difficulty the blood had to escape through the contracted mouth of the aorta, and the *bruit de râpe* was distinctly heard; the woman died, and here is the preparation of her heart; you see what a small passage was left for the blood through the semilunar valves; the fluid could not, consequently, be discharged with any degree of quickness from the left ventricle; hence there was no *redressement* of the point of the heart, no shock against the ribs, and, finally, no production of the first sound; as to the *bruit de râpe*, we must defer its consideration until we speak of the abnormal sounds of the heart. In this case, Gentlemen, you see another proof of the perfect accordance which exists between our theory, and the pathological phenomena exhibited by the heart in various diseases.

Here is another example of a disease of the heart which we were fortunate enough to diagnose at an early period. In our former case, if you remember, it was the first bruit which was wanting; but in the one I am about to speak of, the first sound was sufficiently distinct, while the second was converted into such a confused murmur, that we were justified in considering it as altogether absent. The subject of this case was also a female. As I have just said, the sound produced by the shock of the heart's point against the parietes of the chest persisted, but instead of the second we heard a *bruit de frottement* or *de râpe*, which was evidently contemporaneous with the entry of the blood into the ventricles. It now remained to seek the explanation of this phenomenon,—to account for the replacement of the second sound by one so totally different; for this purpose we had merely to take our theory, and apply it to the case before us, when we obtained the following solution:—To

give the second bruit, it is necessary that the anterior surface of the ventricles should strike against the sternum with a degree of rapidity sufficient to produce a shock; if this be not the case, if any circumstance should occur to prevent the sudden dilatation of those cavities, the shock becomes too feeble to elicit any sound; hence we concluded that some obstacle existed at the ventriculo-auricular openings, which prevented the free and rapid entry of the blood into the ventricles, and consequently rendered the heart's shock too soft and prolonged. This application of our theory, Gentlemen, proved perfectly correct. After death we found a remarkable contraction of the right auriculo-ventricular orifice, and in consequence of this pathological disposition, the blood was impelled slowly and with difficulty into the right ventricle, producing a murmur, but no shock, and consequently no distinct sound.

Here, Gentlemen, is a pathological fact which cannot be explained by any other theory than the one which I have had the honour of proposing to you; I defy you to comprehend otherwise why, in some cases, the first sound of the heart persists, while the second has disappeared; or, on the contrary, why, in other cases, the first bruit is null, and the second audible enough. Remember, that I speak at present merely of the normal sounds of the heart; the pathological we shall leave for a future occasion. I repeat again that the modifications of the natural sounds of the heart, and the various phenomena connected with them, cannot be understood by any system hitherto taught in the schools, but receive an explanation only from the theory of the shock.

But it is not enough to speak so generally as we have done on the two bruits of the heart. Let us now examine each somewhat more minutely, and see if we can discover any physical circumstances which account for the particular characters it exhibits. You know there are two distinct sounds or bruits; one clear and brusque, analogous, as Laennec says, to the clack of a bellows-valve, the other dull and prolonged. Let us apply ourselves to the causes of the difference, and try if we may be able to explain why one is sharp and clear, while the other is long and dull.

The first sound of the heart, the dull prolonged one, is, as you are now prepared to admit, produced by the contraction of the ventricles, giving rise to redressement of the whole organ, and a shock of its point against the ribs; it is thus developed by a muscle in a state of contraction, and hence more elastic than it otherwise would be. Again, the cavities of the ventricles at this moment are almost obliterated, and contain a very small quantity of blood; and, finally, the point of the heart strikes against a bony part of the thorax, which is covered by several layers of muscle, and in addition to them, in many persons, by a considerable quantity of fatty matter. These several circumstances, Gentlemen, readily account in my mind for the dulness which characterizes the first sound of the heart; in fact, the part of the chest against which it strikes is by no means sonorous, or at least is much less so than on the median line. Of this you can easily convince yourselves, by percussing over the interval of the fifth and sixth ribs, and then over the region of the sternum; the difference of sound is most striking. Thus, in the structure of the parts surrounding the heart in its different states, we find certain physical conditions which account for the difference in the character of the normal bruits emanating from the organ, for consider what happens during the production of the second bruit: here the circumstances are quite changed, and the results accord with the change of condition. The heart itself, instead of being contracted into a hard elastic body, is in a state of the utmost dilatation: its fibres are separated and elongated in all directions; the cavity formed by the ventricles has reached its greatest extent; the blood enters suddenly, dilates the parietes of the ventricles, and forces them with a rapid shock against the posterior surface of the sternum. If the stethoscope, or naked ear, be held over the part during the diastole of the ventricles, the sound produced by the shock will be clearly distinguished. Remark, also, the difference of parts against which the heart strikes in the two cases: here you find a flat, hard, thin, sonorous body, which one might compare to a sounding-board, and against which the heart, now hollow and distended, strikes with force. It is not covered, like the rest of the thoracic parietes, with muscle, or layers of fat, but presents a bony and very sonorous surface to the shock of the organ; hence the sound, arguing *a priori*, ought to be clear, and this, as you all know, is actually the case.

Thus, Gentlemen, you have an illustration of a doctrine developed in an early part of our course, and which we have never lost sight of—viz. that various phenomena, hitherto supposed to be vital, are merely the result of certain physical conditions. In this case we have pointed out to you the conditions accompanying the two sounds of the heart, and demonstrated that each physical state is accom-

panied with its own peculiar sound, which is more or less modified, according as the physical conditions enumerated may be altered by disease or otherwise.

If the time permitted, I could cite numerous pathological facts in support of this opinion; at present we must be content with quoting one from LAENNEC; who says that the bruit produced by the pulsations of the heart is clear, in proportion as the parietes of the ventricles are thinner, and that the sounds which we hear in hypertrophy, and in the contrary state, are quite distinct. In cases of moderate hypertrophy, we hear nothing but a low confused bruit, and when the disease has arrived at a certain point, all sound disappears completely. On the contrary (says LAENNEC), when the parietes of the ventricles are thin, the bruit produced by their contraction is clear and very sonorous. LAENNEC makes no attempt to account for these phenomena, which he has observed with so much accuracy, but nothing can be more natural than their explanation according to our theory.

Thus, Gentlemen, you see, that whether we consider the simple physiology of the heart, or pass to its various and complicated pathological conditions, we obtain an explanation of the different facts presented to us, by supposing that the bruits of the heart are produced by the shock of the ventricles either against the parietes of the thorax or the sternum.

Having thus disposed of the normal sounds, it remains for us to fulfil a much more delicate and difficult task,—to follow our subject to the pathological sounds, and see whether we may be able to explain them also in the same manner; but we must defer this to another lecture.

BARON DUPUYTREN, at once the most experienced, the most learned, and the most acute of European surgeons, whose health has been greatly impaired for many months, and whose longer existence was in a doubtful state during the whole of the past week, departed this life at his house in the Place de Louvre, Paris, on Sunday morning last, the 8th inst., at four o'clock, in the fifty-sixth year of his age. The melancholy information has not reached us early enough this week to allow us to collect such materials for a biographical sketch of this renowned member of the profession, as are calculated to afford interest to our readers, but we hope to assemble them in an early Number.

CLINICAL MEDICINE.

LECTURES

DELIVERED AT THE

NORTH LONDON HOSPITAL,

BY

A. T. THOMSON, M.D.,

Physician to the Hospital, and Professor of Materia Medica in the University of London.

LECTURE X.

PROGRESS OF CASES NOTICED IN PREVIOUS LECTURES.

Hypertrophy, with Valvular Disease.—THE great advantage, Gentlemen, which has been derived from the use of the hydrocyanic acid, in the case of hypertrophy with valvular disease, noticed in my last lecture (page 669), would almost lead me to doubt the correctness of my diagnosis, had I not before witnessed similar intervals of quietude follow the use of that powerful sedative in this state of the heart. Schaaf has left the hospital so much relieved that he can lie down in bed; and he has lately experienced none of those sensations which, before he took the acid, frequently awoke him, and obliged him to sit up in the night; he has, also, during the same time, had no palpitations; and the rasping sound, on the passing of the blood through the mitral valves, although it was still heard when he left us, yet was comparatively obscure. I attribute these favourable changes to the salutary influence of the acid on the digestive organs, and the consequent removal of all irritation arising from crude and undigested matters in the alimentary canal, although we must not lose sight of the advantage derived from the perfect rest of body and serenity of mind which the poor patient enjoyed during his stay in the hospital. What may be the permanent extent of the benefit thus procured for our patient, it is difficult to prognosticate.

Phthisis, treated with Emetics and Tonics.—At our last meeting I informed you of my intention to add to the plan of occasional small bleedings, and the administration of tonics and narcotics, the use of a direct emetic twice or three times a week, in Jarvis's case (page 671). On the 29th, therefore, he was ordered to take gr. xv of the sulphate of zinc in solution in the evening; it did not vomit him, but

passed the pylorus and operated on the bowels, and he complained much of debility on the following day. On the evening of that day, however, the 30th, he took ℈j of the sulphate of zinc, which operated as an emetic. Two days afterwards the report was, "He is much better, the cough is less frequent, the chest altogether easier, and the countenance greatly improved." The change in his face was, indeed, very striking; the skin was clear, and the expression of anxiety which had previously settled upon it was not perceptible, and he declared that he felt better generally than he had done for many weeks before. He repeated his emetic that evening, and has taken it once since, and has been cupped once, continuing, at the same time, the use of the hydriodate of iron three times a day, and the narcotic at bed-time. The only comment that is required I leave to be made by yourselves on examining the patient, and observing his improved condition.

Neuralgic Affection of one Side.—Comparative advantage of Belladonna and Aconitine.—Finding that we were gaining little ground in Langridge's case (page 643), I was induced to try the influence of topical narcotics on the pained parts, and, therefore, I ordered a plaster, consisting of ʒss of extract of belladonna, mixed into a paste, with bread and water, to be applied to the forehead, and the use of the carbonate of iron to be continued. On the following day she stated that she had felt some abatement of pain since the application of the plaster; there was, however, still much tenderness on pressure. Having thus experienced the advantage of the belladonna, I was disposed to extend its beneficial influence to the arm, and, consequently, ordered a liniment composed of ʒj of the extract of belladonna, ʒj of tincture of opium, and f.ʒjss of olive oil, to be assiduously triturated together, and the mixture to be applied by friction to the arm at bed-time. On the 28th of January, two days afterwards, little benefit had been derived from the use of the liniment; she still complained of much tenderness on pressure of the left arm and leg; neither was there much abatement of pain of the scalp, and the forehead was puffy and a little red. As the pulse was sharp, she was ordered to be again cupped behind the ears; and the bowels were directed to be cleared by means of a black dose in the morning. The carbonate of iron was continued. On inquiring into her feelings on the 30th of January, I found that the belladonna was productive of one inconvenience,—it caused a temporary loss of vision; I, therefore, determined to change it for the ointment of aconitine, which has been extolled in neuralgia. It is made with gr. viij of the aconitine, and ʒj of lard. On the 2nd she complained of the ointment causing a pricking sensation soon after it was applied; and she stated that it had failed to afford her that relief from pain which she had experienced from the application of the belladonna plaster; the latter was, therefore, ordered to be again applied. The aconitine had previously been used in the case of Howell, but, instead of affording relief, it had augmented all his sufferings, and caused a sensation, to employ his own language, "of needles running into the limb." As these are the only two cases in which I have seen the aconitine employed, I do not feel that I am authorized to offer an opinion respecting its influence in neuralgia. Neither of these cases can, perhaps, in strictness of terms, be regarded as neuralgia, although the symptoms evidently depend on some morbid condition of the nerves, which has augmented their susceptibility of impression to a morbid degree.

SMALL-POX.—*Inlets of Infection.*

The only case to which I wish at this time particularly to direct your attention, is one of small-pox, which, although distinct, yet has run its course with considerable severity. The patient is John Male, aged thirty-five years, of a sanguine temperament. He was admitted on the 25th ultimo. When Male entered the hospital, the eruption already covered the face and wrists, and was thinly scattered over the shoulders. A general efflorescence, or roseola, accompanied the papulæ, which already displayed the central depression, and left little doubt of the real nature of the disease. It may be unknown to some of you, Gentlemen, that this central depression of the pustule, which is conspicuous at its first appearance, and is one of the most characteristic features of the disease, depends on the cellular organization of the pustule, which is seated in the cutis vera, and consists of from six to eight cells, tied together in the centre. This appearance generally continues till the sixth day of the eruption, after which the central band gives way, and the depression disappears, as the pus is increased in quantity and maturated; the pustules then become turgid, and assume a somewhat globular form, slightly acuminated. In our patient they are less pointed, and unusually large and turgid. At first the areolæ, instead of being of a bright red, displayed a purple hue, but this changed to red as the maturation ad-

vanced. The patient had experienced a rigor on the morning previous to his admission, with an acute pain of the epigastrium, nausea, and a vertiginous sensation, which continued at the time of his admission, for he staggered in his walking, and complained greatly of prostration of strength. He had been told by a practitioner who had seen him, that his disease was scarlet fever, which alarmed him. The pustules were soon developed in great numbers on every part of the body; they rose well and were distinct, except in a few spots upon the face and arms, in which they were so closely grouped as to appear confluent. There was not much swelling of the face, and scarcely any delirium had been observed.

As small-pox is now less frequently seen than formerly, I am anxious to direct your attention to the declining stage of the disease, which has already commenced, and to observe that, even although this case cannot be termed confluent, yet, in the spots where the pustules coalesce, you will find that the cicatrization will probably be attended with pitting, or a loss of substance in the skin. The coalescing in this case, as in all others, is most striking in the face. There is scarcely any secondary fever; and I do not anticipate more than at present exists.

From the nature of the symptoms, and the progress of the disease up to the present time, the prognosis could only be regarded as favourable, although the confluent state of the pustules on the face left a slight shade of doubt; and this would have been greatly increased if the areolæ had continued to display the purplish hue which I have mentioned as at one time existing. There is neither cough nor much hoarseness, and although the nights are sleepless, yet there is none of the restlessness, tossing, moaning, and desponding, which always indicate danger.

With regard to the treatment of this case, as far as it has proceeded, little more has been done than keeping the ward, in which the patient is placed, cool; regulating the bowels; enforcing starvation, and directing the administration of saline refrigerants. Two days ago, as I found the pulse flagging, probably owing to the drain upon the habit, from the great number of the pustules; and as the nights were sleepless; I directed that he should be moderately supplied with weak white wine whey, and that an opiate should be given at bed-time, after opening the bowels with castor oil. In what remains of the disease there is nothing to fear, if the profusion of the pustulation do not so much depress the powers of life as to cause the formation of sloughy sores upon the points on which the body rests. To prevent the infection from spreading in the hospital, a fumigation of chlorine has been placed in the ward.

One reason for my bringing this case before you, is to enable me to mention my opinions on the nature of the virus of infectious diseases, and on the inlets by which infection is received into the body,—a subject of considerable interest; and which, although in some measure theoretical, yet, with the opportunities before you, presents a study of much importance in a practical point of view. On this subject, three propositions are generally admitted:

1. Experience fully warrants the belief that the virus which produces certain diseases, is held as an effluvium, or invisible intactile agent, in the air, whether it emanates from soils in which animal and vegetable decomposition is going forward, or is exhaled from the bodies of the diseased.

2. That from observation we are authorized to infer, that there are different species of morbid effluvia, each of which is capable of producing a distinct disease.

3. That observation, also, has confirmed the opinion, that a certain space of time intervenes between the period when the effluvium is applied to or introduced into the habit, and the appearance of the disease.

Now three subjects for consideration arise out of these propositions; the first relates to the nature of the effluvia; the second to the manner in which they are introduced into the system; and the third to the probability that some of them cause the diseases which proceed from their influence, altogether independent of their introduction into the body.

With regard to the nature of morbid effluvia, chemistry affords us very scanty information. It leads us, however, to conjecture, that some of them are of an alkaline nature, from the facility with which they appear to be neutralized by nitric acid vapour; and that others consist chiefly of hydrogen, or its gaseous combinations, from the effect of chlorine upon them; and, also, from the formation of ammonia, when putrefying animal matters are present in the places where they exist to supply nitrogen. But still it is uncertain whether the substances which are thus evolved be themselves the virus of the diseases produced during their presence, or whether they are merely the solvents or media by which the noxious effluvia are diffused throughout the air.

Too few observations have been recorded to enable us to form even a satisfactory hypothesis upon this part of our

subject; nevertheless chemistry may afford some obscure lights to guide us in our researches. If we reflect upon the chemical nature of the principal productions of stagnant pools and fenny soils, such as are justly regarded as affording the exciting causes of intermittent and remittent fevers, we may be enabled to deduce some probable inferences. In such places, we know, that the cryptogamia abound; and that whenever this is the case in stagnant water, there is a large extrication of carburetted hydrogen, which operates as a powerful sedative poison, when applied to the living system. Whether this, when diluted as it must be in atmospherical air, is capable of commencing that series of symptoms, the catenation of which constitutes ague, I am not prepared to assert; but I am strongly disposed to believe, that that fever is the consequence of the impression of some purely sedative agent on the sentient extremities of the cutaneous nerves. It has been supposed that humid air itself is capable of producing such an effect, seeing that intermittents occur at sea in foggy weather, when no land is in sight. But, independent of the probability that the fog has travelled from the land, and brought with it the miasm of the disease, it is not impossible that the fresh aqueous vapour precipitated upon the deck, in an atmosphere of diminished density, may so operate upon the deck as to decompose the wood, and to produce the same exhalation which emanates from stagnant water and fenny soils. All this however is, as yet, matter only of conjecture.

If little satisfaction has, hitherto, resulted from our researches into the nature of miasmata, much less has followed from the few investigations which have been instituted into that of fomites, and the immediate exhalations from the bodies of those labouring under infectious diseases. From the experiments, however, instituted by Dr. HENRY, into the disinfecting agency of heat on the contagious matter of cow-pox and scarlatina, there is some ground for supposing that the virus emanating from the body of any person labouring under infectious disease, is a compound substance; and that it is decomposable by a temperature not under that of 200 Fah.; but although this inference may be drawn from the destructible nature of the *fomites* of cow-pox and scarlet fever, yet experiments are still wanting to extend the proof to other species of morbid animal effluvia.

Upon the whole, science has little to boast of in its attempts to investigate the nature of infectious effluvia.

In our examination of the second head of our subject, the manner in which these infectious effluvia are introduced into the system, we must set out by assuming it also as admitted that there are only three inlets by which that can be effected: the *skin*, the *lungs*, and the *stomach*. Now let us, first, ascertain how far each of these organs is adapted for performing the function of imbibition.

With regard to the general imbibing power of the skin, much difference of opinion exists amongst physiologists; and the inferences drawn from the results of the same experiments, by different experimentalists, are the most opposite and contradictory. The experiments by which HALLER conceived that he had clearly demonstrated that the body gains weight by immersion in the warm-bath, and, consequently, that the skin imbibes aqueous fluids, were repeated by SEGUIN, who deduced from them a directly opposite conclusion. His opinions, in a great degree, swayed the reasonings of physiologists, until they were refuted by those of Dr. CURRIE of Liverpool, who confirmed the accuracy of HALLER's conclusion. The most satisfactory of his experiments is one made on a case of dysphagia, in which, although no weight was gained by the immersion of the body in the warm-bath, yet the loss continually going on in air, was suspended during the immersion. Besides, the discharge of urine, subsequent to the employment of the bath, alone exceeded much, in weight, the waste of the whole body; and in other respects the other ejecta, the stools and perspiration also, exceeded the injecta, which consisted of glysters, the weight of which was easily determined. Could this occur except by cutaneous imbibition? If this be admitted, the following question still requires a reply—is the imbibition affected by the whole surface, or confined to certain parts? Were we to judge of the function of the skin, from the innumerable concourse of absorbents which cover the *cutis*, we could not hesitate to pronounce it to be an imbibing organ at every part of the body; and if the cuticle be removed in any part, this is demonstrated to be true, by the rapid absorption which ensues. The cuticle is the great impediment to cutaneous imbibition. Some experiments, however, of Dr. ROUSSEAU, of Philadelphia, afford reason for thinking that the function of imbibition is enjoyed only by certain portions of the skin; that space, for instance, which extends between the middle of the thigh and the hip, and that between the middle of the arm and the shoulder. When poultices of rhubarb, or of madder were applied to those portions of the skin, the colouring matter could be detected in

the urine, but not when they were applied to any other part of the surface. It is not easy to explain this fact, as no anatomical difference is apparent in the cuticle of these portions of the skin; it readily, however, accounts for the circumstance, often observed, that many poisons, such, for example, as the virus of syphilis, and the venom of the viper and of the rattle-snake, may be handled with impunity, if the cuticle be entire.

But as the matter of infection is an effluvium, and contained in the air, it is requisite to ascertain whether vapours and elastic fluids are absorbed by the skin. Many experiments, especially the following by Dr. Rousseau, induce us to believe that this is not the case. He filled a room with the vapour of oil of turpentine, and breathed through a tube which was closely fitted to his face, and communicated with another apartment; although his naked body was immersed in such an atmosphere, yet no violet smell could be detected, either in the urine or in the breath, indicating the presence of turpentine in the system; nor was any discovered even when the body was sponged with oil of turpentine, or with the juice of garlic. It is true that the system has been frequently charged with mercury by mercurial fumigating baths; but, in these cases, the fumigations have not been made with such precautions as to enable us to determine whether the volatilized mercurial preparation was admitted by the skin, or through the lungs, or by the medium of the saliva, and, consequently, into the stomach.

Upon the whole, I am of opinion that the skin cannot be regarded as an inlet of infectious effluvia to the system, although, as I shall endeavour to prove, it may be the medium on which miasms may impress disease on the habit, and by which infectious matter may influence the system, and produce in it the disease which the person laboured under from whose body it was exhaled. A question may be here started—namely, are the exanthemata the results of poisonous effluvia acting on the cutaneous capillaries? We know that tartar emetic, applied to the surface, causes pustules; mercury, a vesicular eruption; and the stings of nettles, wheals resembling those of urticaria.

The next inlet open for the effluvia of infection is the lungs. It is not easy to ascertain the imbibing power of those organs, without placing them in an unnatural situation, although, if the inferences which may be drawn from the state of the skin be correct, we may ascribe many of the results which are attributed to the action of the surface, to that of the lungs. Thus, when we are informed by De Haen, Haller, and others, that persons have, for long periods of time, micturated more than double the quantity of fluid received into the stomach, and that weight has been gained when neither food nor drink has been taken, we can scarcely suppose that the fluids which have supplied this can have entered by the skin. "Virgo nobiles," says Haller, "quotidie 260 uncias urinæ edidit absque cibo et potu." Keil remarked that, in one night, his body had gained eighteen ounces; the Abbe Fontana, after exposure for two hours to a moist atmosphere, returned home some ounces heavier than when he went out; and the late Dr. Home, of Edinburgh, having gone supperless to bed after much fatigue, found that he had gained two ounces during the night. The most remarkable instance of this kind, however, is that related by Dr. Percivall of a horse jockey, whose weight increased thirty ounces, although, in the interval, he had taken nothing but one glass of wine. Now, as I have already said, it is more probable that moist air conducted into the lungs in inspiration should effect these augmentations of weight than that they are the consequence of cuticular imbibition; and this opinion receives confirmation from the known effects of introducing fluids of various kinds into the lungs of quadrupeds.

Professor Mayer, of Berne, injected solutions of rhubarb, saffron, indigo, madder, ferro-cyanate of potassa, nitre, alum, acetate of lead, and muriate of iron, into the lungs of rabbits, dogs, cats, goats, and some other animals, and found that they were absorbed and carried into the blood, and could be detected there before they were apparent in the chyle. Ferro-cyanate of potassa was detected in the urine, by testing that secretion with persulphate of iron seven minutes after the ferro-cyanate was injected into the trachea. Prussiate of potassa injected into the lungs was first detected in the arterial blood of the heart and arteries, and afterwards in the venous blood; it was also detected in the serum of the pericardium, that of the pleura and the peritoneum, in the synovia, in the milk, and in the cellular fluid under the skin. Such are a few of the facts proving absorption of fluids by the lungs.

With respect to vapours and gases, Dr. Rousseau found that by inhaling the vapour of oil of turpentine placed in another room through a long tube connected with the nostrils, the violet odour soon became apparent in the urine; and a similar result followed the inhaling the same vapour by the nostrils from a narrow-

necked bottle; and also when he emptied the lungs, and inhaled the vapour through the mouth by a full inspiration. With regard to gases, Dr. Edwards found that both oxygen and nitrogen are absorbed in the lungs.

Now, if we admit the matter of infection to be held in solution in the air, we may observe that the system may become infected through the lungs in two ways. The infectious matter may act upon the extreme branches of the eighth pair of nerves which supply the bronchial cells, and a new action set up in the habit independent of absorption, by the impression thus made being directly conveyed to the great nervous centres, the brain and spinal marrow; or the gaseous virus may be imbibed, as in the case of the fluids thrown into the trachea, and thence be conveyed into the circulating mass, operating in this case in the same manner as if it had been introduced by inoculation. It is probable that the pulmonary exhalation of phthisis, in the last stage of the disease, becomes infectious, and operates in the former manner, exerting its influence first upon the lungs themselves, and afterwards upon the constitution; and that the infection of some diseases, as, for example, hooping-cough, never passes beyond the lungs. Indeed, if we take every circumstance into consideration, and reflect upon the nature of the lining membrane of the bronchial tubes and cells, which is much more likely to permit of imbibition than the cuticle, there is every reason for regarding the lungs as a much better inlet for the matter of contagion than the skin. It is probable, also, that it is admitted rather by simple imbibition, than by true vascular or lymphatic absorption.

The third inlet of infection is the stomach. Infectious effluvia may be entangled in the saliva, and this being swallowed, loaded with the virus, its first influence is produced upon the nerves of the stomach; its second upon the system generally, by absorption. The possibility of morbid poisons, in an aeriform state, being introduced by this inlet, must certainly be admitted; but this channel is undoubtedly less direct than that of the lungs. The manner in which the poison is introduced is evidently not that by which nutriment is conveyed, for neither odorous nor colouring matters are detected in the chyle, although they have been found both in the blood and in the other secretions. The experiments of Flandrin, Tiedemann, Gmelin, and Magendie, have clearly demonstrated this fact; and it can only be accounted for by the probability that some of the lacteals do not enter the thoracic duct, but proceed directly to the vena portæ, and thus carry the matters taken up from the alimentary canal directly into the veins. Meckel, Lobstein, Lindner, Sir Astley Cooper, and others, have demonstrated that quicksilver injected into the absorbents of the stomach and the intestinal canal, finds its way directly into the vena portæ. Magendie has also proved that poisons are taken in by the same way, for having tied the thoracic duct of a dog in the neck, and having introduced some powdered nux vomica into the stomach, he found that the poison acted in the same manner as if the duct had not been tied. But although it must be admitted that some infectious matters may enter the system through the medium of the saliva, carrying them into the stomach, yet when we consider the nature of the vehicle by which they must be introduced, the influence of the digestive function, and its power of resisting the action of some fluid animal poisons, namely, that of syphilis, small-pox, cow-pox, and the venom of poisonous serpents, there is much room for concluding that the stomach is not a common inlet of infection. Indeed, on a review of the whole argument, the lungs, it must be admitted, are most frequently engaged in this office.

If the virus of small-pox be not introduced by inoculation, it can only be communicated through the medium of the air, and admitting this, it becomes a subject of much practical importance to ascertain, if possible, the state of the atmosphere most favourable for its propagation. Hitherto, however, no information has been obtained that can throw any light upon this inquiry, except that the condition of the atmosphere, whatever it may be, which favours the influence of variolous infection, recurs at nearly stated periods; and, at these times, the disease becomes epidemic. A point which has been more accurately determined, is the period of the disease in which the infection is most communicable. It is well known that for the first two days after the eruption appears, there is little to be feared from infection, although even during the premonitory fever, immediate contact may communicate the disease. The infectious property continues whilst a single scab remains; and it is said, although I have had no experience of the correctness of the remark, that the infection, in very bad cases, may be communicated by a body that has been dead for some days.

In concluding these remarks, I have only further to add, that as experience has fully demonstrated that vaccination is not always a security from variolous in-

fection, you should be aware of the importance of maintaining the tone of the habit whilst attending such cases. A plethoric and heated condition of the body, from intemperance or full living,—and a state of debility, either from mental or bodily circumstances,—are equally predisposing causes of susceptibility to the infection of small-pox. In this, as in all cases of infectious diseases, also, the practitioner should examine his patient standing on the windward side of the bed, and wash his hands as soon as possible after his visit.

CLINICAL SURGERY.

LECTURES

DELIVERED AT THE

NORTH LONDON HOSPITAL,

BY

ROBERT LISTON, Esq.,

Surgeon to the Hospital, and Professor of Clinical Surgery in the University of London.

LECTURE VII.

DISEASES OF THE RECTUM.

Spasmodic Contraction of the Anus from the Irritation of Foreign Bodies, and Fissures and Ulcers at the Orifice.—Stricture of the Rectum; its Rarity.—Danger of using Bougies and other Instruments.—Close Approximation of Stricture of the Rectum to the Sphincter Ani, its Characters, attendant Circumstances, and Treatment.—Double Stricture.—Scirrhocontracted Rectum.—Strength of Local Remedies.—Objections to Operations in these parts.

GENTLEMEN,—Having finished those remarks on diseases of the lower bowels attended by and arising from dilatation and relaxation of the parts, which suggested themselves as likely to be useful to you in practice, I must now turn your attention to a different set of cases, those in which distress is occasioned from narrowing of the passage. This is met with at the very outlet, as in the urethra and other mucous canals. There is occasionally seen a permanent and organic stricture here, the orifice forming a hard, narrow, and unyielding ring. This arises from long-continued irritation of the part, immediate or remote, and consequent change of structure, from the cicatrization of rhagades (deep fissures); or it is seen to follow loss of substance, as after some of those unwarrantable incisions, excisions, or cauterizations, which are occasionally practised on these parts.

It is not likely to be alleviated, in the end, by any further loss of substance, as has been recommended. The contraction in this situation is more frequently spasmodic, and referable to irritation, either local, or in some part with which the extremity of the rectum has a close sympathy. The sphincters have been blamed, have been accused of being naturally too strong in some subjects. The vice is to be searched for elsewhere. The most troublesome case of this contraction of the anus which I have ever met with, arose from the lodgement of this piece of bougie, which had rested in a sinous track in the neighbourhood of the anus for many years. The circumstances are recorded in the catalogue of my pathological collection, and I have extracted them from that source, with very little alteration:—

"Patient, a male, ætat. 40. Had suffered under a long tortuous sinus of the hip, extending towards the anus, for nearly eighteen years. He was also affected with stricture of the urethra. A large bougie had been introduced deeply into the fistulous opening whilst under treatment in Holland, and could not be extracted. The sinus in consequence continued open, causing at times great pain, itching, contraction of the sphincter, much nervous irritability, and distention of the stomach. The patient had consulted many eminent surgeons in England and France without much relief. The sinus was laid open as far as a probe could reach. Some time afterwards, on dressing the wound, a hard substance was perceived by means of the probe, firmly adherent to the parietes of the upper part of the sinus. This, after having been extracted, proved to be a portion of a bougie, with many hairs on it. Then, only, the introduction of it was mentioned, the patient having been assured that it would undoubtedly be *absorbed*. The wound healed rapidly, and the patient obtained a complete cure."

This patient was relieved immediately of many of his most troublesome symptoms on the removal of the foreign body, and was perfectly cured, after getting rid of a very bad stricture of the urethra, produced and aggravated no doubt by the state of parts near the fundament. The lodgement of foreign bodies—pins, bones of fish, or of small animals—in the coats of the intestine, or in the neighbourhood of the anus, will give rise to the same feelings. These bodies sometimes pass through the whole track of the canal, and are here arrested; or they may be applied directly. Some strange enough cases are recorded of the introduction of substances into the

bowel. You must all know well the case related by MARCHETTI, of the pig's tail, with the bristles cut short, which was introduced maliciously into the rectum of a poor woman, and you cannot have forgotten the ingenious plan which he fell upon to extract it. The case is to be found in most of the works on the subject of diseases of the rectum, since it was first published in the Memoirs of the French Academy.

The most common cause of the spasmodic contraction of the sphincters (by some set down as a consequence) is the presence of chaps or fissures in the orifice. This troublesome complaint is described by PARE and WISEMAN, but the attention of the profession has been directed more particularly of late years to the subject by Baron BOYER. These rhagades or clefts are met with of various depths, singly or not; the one end of them is visible, but their full extent is not ascertained, unless when the patient presses down; they generally rise in the direction of the bowel, but sometimes deep ulcerated cavities are found within the sphincter, and running transversely. Such breaches of surface no doubt frequently have their origin in laceration of the membrane, by the passage of hardened excrements or of foreign bodies. Large pieces of bone appear to pass along the whole track of the intestinal canal without difficulty or inconvenience, and are stopped by the sphincter. I have removed many (the last was nearly one half the jaw of a rabbit) from within its grasp. You can readily understand how the existence of such breaches of surface should occasion and keep up uneasiness and spasm in the outlet of the canal. The regulation of the bowels, the clearing out of the lower part of them, careful ablution, must all be attended to in such cases. The irritability of the sores may be done away with and their healing promoted by the occasional use of the nitrate of silver, in substance or in strong solution, applied by means of a camel-hair brush. The sores within can be readily seen and reached in most cases, so that remedies suited to their state may be employed. Some of the French surgeons have great faith in an epithem composed of extract of belladonna and acetate of lead in lard. I have had no experience of the effect of this remedy, and should not expect much benefit to arise from it. The nitrate of silver applied efficiently (not as an escharotic) and at proper intervals, generally answers the purpose, and leaves nothing further to be desired.

In bad and intractable cases, in ulcer within the verge, it may be necessary to follow the practice recommended by BOYER, which I have followed successfully in a few cases, and which was alluded to in considering the case of warty growth attended with ulcerated cavity; I mean the division of the sphincter, with the view, then mentioned, of putting the parts at rest for a time. The incision is readily made with a blunt-pointed straight and narrow bistoury, conducted on the finger; the incision may be made through the fissure or ulcer, if that be towards the one or the other side. It is immaterial whether the chap is involved or not in the little operation. It has been proposed by some one or other of the rectum-doctors to take out a piece of the verge of the anus for the cure of contraction, and this he recommends to be effected with such a cutting scoop as cheese is cut with: whoever resorts to a proceeding of this kind, must have paid little attention to what results from loss of substance in this or any other situation.

Now, stricture of the rectum, properly so called, is, upon the whole, rather a rare disease.

I can give you no stronger evidence of the rarity of stricture in the rectum, unconnected with any specific disease, such as scirrhus or venereal affection, than the fact, that in all the pathological museums taken together in this vast metropolis, you will find but very few specimens. The same holds good in regard to collections in other countries; and those old practitioners with whom I have conversed on the subject, and whose testimony I value, all agree as to the extreme rarity of such cases. The fact is that the very frequent existence of this disease has been asserted now and then to serve no very good purpose. I have been assured, on very good authority, that at a fashionable watering-place, patients have been persuaded, whatever the complaint might be that they were afflicted with, that their sufferings depended upon a stricture of the rectum. But it is not at watering-places only that such charlatanerie is practised; and as most invalids experience difficulties in evacuating the bowels, many become victims to the most disgusting treatment. So successful was the imposition, and so flourishing the trade, at one time, that a patient informed a friend of mine, that when he went for advice, he was ushered into a room where there were no less than eleven individuals, each with a bougie in his fundament, and with the small-clothes cut after a particular fashion, to facilitate their easy application.

The system of using bougies for the rectum, for the cure of supposed or fictitious stricture, and for all sorts of complaints, has in some cases been attended

with much more serious consequences than the mere extraction of money. Not a few patients have succumbed rapidly within a very few days after the examination made with bougies and ball probes. "Let it be remembered," says Mr. COLLES, in a very good paper, to which I have before alluded, "that serious mischief has occasionally been committed by rude attempts to dilate a *supposed* stricture at the top of the rectum or termination of the colon; for I have known two cases where peritoneal inflammation and death speedily ensued; and I have heard, on the best authority, of two similar instances." Whether there was actually lesion of the bowels and extravasation of their contents into the peritoneal cavity, or whether inflammation was propagated from the injured parts to such an extent as to prove fatal, I have had no opportunity of ascertaining.

That such will now and then happen, is shown by a case related by Sir C. BELL, in a lecture on this subject. The patient was anxious to get rid of a very troublesome complaint, a false passage opening from the urethra into the rectum. The patient himself persisted in dilating the bowel, so as to afford a proper view of the opening; and from retaining the instrument for too long a time, peritoneal inflammation supervened, and proved fatal in a very few days. A recollection of these facts will teach you caution in the exploration or treatment of disease in this part.

Some of those who have written on this subject, Mr. COPLAND, Mr. MAYO, and others, indicate clearly the seat of stricture, but they, at the same time, would make it appear that obstructions exist high in the bowel, of a spasmodic or permanent nature; and for this they recommend the very uncertain and unsatisfactory means of relief by instruments of various kinds, which they would have introduced towards, or into, the sigmoid flexure. I by chance stumbled on one case (the patient himself, a medical man, relates it) in which "a bougie, *three feet* in length, was considered necessary to reach the stricture," and was thrust into his bowels accordingly. He got tired of the system at last, and got well by attention to diet, as he would have done in the first instance. I leave you to judge what satisfactory information or advantage could accrue, either to the doctor or the patient, from the proceeding above detailed. The object, the *avowed* object of many of the lucubrations on this subject, is "to prove that stricture of the rectum is *a very common disease*," and that its seat is far beyond the reach of the sight or touch. A great deal is said also about "spasmodic action" and "permanent spasm" in some of the books I have looked over. The proper signification of terms has not been much considered by these authors. The disease to which the term of "stricture of the rectum" can with any propriety be applied, is met with immediately within the sphincter ani. It is a veritable contraction of the whole circumference of the bowel, varying in tightness, having a sharp edge, and attended with some thickening and alteration of the coats of the viscus for a little way above and below. There is a widening of the canal above, from accumulation, a profuse discharge of vitiated mucus, of muco-purulent fluid, occasionally of blood, from the contracted part, or from accompanying piles; not unfrequently there is ulceration to be discovered above the contraction. Sometimes abscess forms in the surrounding cellular tissue, and makes its way into the bowel, by the side of it, outside of the sphincter; into the vagina in the female, or some part of the urinary apparatus in the male. In some rare cases, abscess has passed through the sacro-ischiatic notch, and formed a swelling upon the hip, through the opening of which, as in the case from which this preparation was obtained, fluid fæces and flatus are discharged. The track or fistula accompanying stricture, generally is *complete*, and opens into the bowel above. The contraction is generally single, but I have met with two cases in which one ring was situated above another, a portion of sound bowel being interposed; in fact there were two distinct strictures, but both within reach of the point of the finger; the upper one could be brought within reach when the patient bore down. The diseased part can even be brought into view, though this is seldom requisite, by the use of a proper speculum, such as I hold in my hand.

In order that you may come to a proper understanding of the seat of the disease, I would have you recollect the form, extent, and course of the "intestinum rectum," or, as my friend Dr. MONRO, of Edinburgh, would have it called, "*curvum*." It is certainly more crooked than straight in its course; the bowel is bent upon itself above the top of the sacrum. It then follows the hollow of that bone, and it is provided with folds or projections of the inner membrane, more or less distinct and prominent, at two or three different parts, immediately within the sphincter, and in the male behind the prostate, at the place where the rectum commences, and where the sigmoid flexure ends, and very often at an intermediate point. These folds are intended, it may be presumed, to retard the excrementitious matters, to prevent that

part of the bowel which is surrounded by the extruding muscles, from being distended, and its action excited, until a convenient occasion should arise. The constant fulness of the rectum, besides, would interfere materially with the functions of the very important viscera in its vicinity. These folds are described by WINSLOW and others of the old anatomists, and a very good representation of a section of the gut is given in one of the volumes of the Dublin Hospital Reports, by Mr. HOUSTON, of which this is an enlarged copy. In the situation of this exterior fold, which sometimes passes around the whole circumference of the bowel, is stricture found to arise. Its origin is readily understood. It is the tightest part of the lower bowels; it is here that foreign bodies stop; it is apt to be hurt by the passage of these or of indurated matters. The action of the vessels is excited, and a deposition takes place in the submucous cellular tissue; the outer surfaces of the folds are agglutinated, and to the subjacent parts. This is increased from time to time by fresh excitement. It is here, as in similar tubes, the gullet, urethra, &c.; the narrowest part is most prone to take on diseased action.

The symptoms arising from, and the circumstances attendant upon, the disease, were well exemplified in the person of A. D., ætat. 27, who was dismissed a few days ago, so much relieved that she desired to be made an out patient. "She was admitted December 15th. She states, that for the last three years, ever since she was delivered of her last child, her bowels have been very much constipated; that about two years ago she became affected with pain, referred to the lower part of the rectum, accompanied with the discharge of a slimy, and, at times, of a bloody purulent matter. She became also affected with tenesmus, bearing-down pains, and, shortly after, with prolapsus recti, which became extremely troublesome, and was occasionally very difficult to reduce. These symptoms, with the exception of the prolapsus, which ceased about twelve months ago, have continued since, and her general health has suffered considerably. She complains chiefly at present of debility, habitual constipation, tenesmus, and also of severe pain on attempting to empty her bowels, which she does incompletely. The evacuations are flattened, and expelled with much straining. The purulent discharge still continues, but has not been mixed with blood for some time past. On examination, which gives much pain, considerable firm induration is felt surrounding the rectum about two inches and a half up, with narrowing of the canal, which does not allow the finger to pass. The indurated portion is not broad, and the bowel on either side, above and below, appears to be pretty sound. She ascribes the complaints to some injury received during delivery. St. h. s. hanst. *Ol. Ricini.*

"16. Bowels freely moved. A rectum bougie was introduced and retained for a short time. The bougie next in size was passed. Feels somewhat easier, and the discharge has diminished considerably. ℞ *Sulphur. S. Tart. Potassæ*, a a ʒj. M. ℥ij bis in dies.

"29. Catamenia have appeared since last report, but have now ceased. A bougie, two sizes above the last, was introduced to-day, and passed with considerable ease."

In such a case as this, advantage was to be expected from the introduction of short bougies. They were used, as you will please to observe, with great gentleness, and by no means very often. The size was increased very gradually, and with much and rapid relief of all the symptoms.

The contraction yielded to this occasional pressure, and advantage was also attributable to the diminution of the susceptibility of the passage. This is only to be expected from the employment of the means at proper intervals. A very few introductions of the bougie thus, will give more effectual and permanent relief, than the constant tampering with the parts,—the daily use and long retention of instruments. In general nothing else is necessary. If fistula be combined, that must be divided, and, with it, the stricture of the inner opening, which is above it. If the contraction do not yield, a few notches may be made in its sharp edge, as practised by WISEMAN, and by some more modern practitioners. I have had patients in whom I think the cure was thus expedited, and more speedy relief afforded. I have already cautioned you as to any rash or coarse use of instruments,—against the thrusting of them no one knows whither. Even the too rapid stretching of a stricture may be followed by obstruction and all the symptoms of strangulated bowel. You can readily understand that this will sometimes be the result of neglected stricture, and that the patient may thus be carried off very suddenly. More frequently he is exhausted by continued pain and profuse discharge; he loses flesh, becomes hectic, pale, and exsanguin; uneasy feelings and swelling of the abdomen come on, and he perishes very miserably.

Now any confinement of the bowels from spasm, or the nature of the contents, must be got the better of by attention to diet, by the use of medicine calculated to allay irritability and promote evacua-

tion, and by enemata of a simple nature adhibited by means of an efficient apparatus. I have said enough, I trust, to deter you from resorting to such explorations with long bougies as I have deprecated, and to put to shame if possible some of those who have set so bad an example before the aspirants to professional reputation. There can be no doubt but that the constant introduction of instruments, the injury inflicted upon the natural folds and upon the coats of the bowel, the laceration and bruising of them against the top of the sacrum, must lay the foundation for indurations, and perhaps for malignant disease.

The occurrence of scirrho-contracted rectum, is, fortunately, also rare; it is a disease which is attended with great and long suffering, even in its early stage, and over which we have little control. The patient may be made, to a certain extent, comfortable during the progress of the malady, by securing an empty state of the bowels, and by soothing local applications, suppositories, and enemata. We are cautioned by Baron DUPUYTREN as to the strength of these remedies. He states that the rectum not having, like the stomach, the power of altering and directing the matters put into it, their action is more prompt and powerful, and that the doses of medicine, put into the rectum, ought to be by a half less than those introduced into the stomach. This does not accord with my experience. Large doses—double the quantity—are required, and do no harm, so far as I have seen. If the rectum have not the faculty of directing and altering, so it has less power of absorption, and less sensibility, than the stomach.

Operations have been practised for the removal of the extremity of the rectum which are highly dangerous and unjustifiable, inasmuch as they are not required for ordinary diseases of these parts, and cannot be followed by any benefit to the patient when the disease is of a malignant character. We are not warranted in performing a painful and hazardous operation, in risking the patient's life, unless there is a fair prospect of ultimate and permanent benefit resulting. Now, even in diseases of the same nature in exposed situations, where we can ascertain the condition of the absorbents, we are too often disappointed in our expectations of producing a radical cure. Here we have no means of knowing the state of the vessels and glands leading from the part. We have every reason to suppose that they must be so far contaminated, and that even though the whole of the original disease could be extirpated, and the patient do not perish, still no good can ultimately come of the attempt. In treating many of these diseases in their advanced stages, and in inaccessible situations, practitioners would do well to recollect the aphorism—"Quibus occulti cancri fiunt, eos non curare melius est, curati enim cito perennt non curati vero longius tempus perdurant." A great many extirpations have been made in this and other regions of the body, very unnecessarily, of parts ulcerated and swelled, which could have been restored to a normal and healthy state by a little attention to general and local management.

I have now very nearly brought my remarks upon disease about the farther extremity of the intestinal tube to a conclusion. Something remains to be said in regard to abscess and fistula. After discussing that subject in reference to the cases under treatment in the wards Nos. 2 and 6, I purpose directing your attention to a set of cases of disease resulting from obstruction to the return of blood in the lower extremities. The consequences of this retardation, dilatation of the veins, inflammation of their coats, swellings and ulcerations, you have had ample opportunity of studying.

I shall endeavour to make it appear that this is one of the most proper, and, to you, interesting subjects that I could possibly have selected. These diseases are of very frequent occurrence, more especially amongst the labouring classes; they incapacitate those who labour under them for active exertion; their treatment is difficult, and very often conducted upon empirical plans. Many are the applications used for breaches of surface, very various in their properties and effects. Many of them are exceedingly useful, but no one plan and no one remedy can be used with equal advantage in all cases. I shall point out to you the characters of the different sores, the changes that take place in them from over or under action, and I shall endeavour to lay down such rules for their management in their various stages, and illustrate this in the treatment, as will enable you to select the means which are most suitable when you shall be called upon to exert your own skill and judgment in these matters. I have chosen the subject from no want of what some might think more interesting subjects for remark.

On joining this institution I was, I must confess, not a little apprehensive that for a time there might be a lack of such cases as would furnish occasion for this most useful form of instruction; and I have

season to suppose that I was not singular in this matter,—that many of the friends of the school entertained the same feeling. On this score there need be now no anxiety. I have set down some heads of lecture, all of which can be well exemplified by cases which are, or have lately been, under your observation. I shall be very much guided as to the order in which these subjects shall be taken up and discussed, by your wishes:—

Wounds of the scalp and erysipelas.

Diseases of the eyelids;—tumours, eversion, granulated conjunctiva, opaque cornea.

Rhinoplastic operation.

Diseases of the larynx;—ulceration and œdema.

Diseases of the urethra;—retention of urine.

Tumours of the uterus.

Diseases of the joints;—of the knee, of the hip.

Necrosis.

Aneurysm from venesection.

Spontaneous mortification;—gangrenous erysipelas.

Fracture;—of the thigh, of the leg, compound and simple.

Mercurial cachexia.

It will cost me no trouble to take into consideration such of these subjects as you wish, seeing that I have discussed them over and over again, both in systematic, surgical, and clinical lectures. Instead of looking upon my duties here as troublesome, I can assure you that the meeting you here from time to time to conduct the treatment of the cases, and explain the principles which guide me, is a source of unmixed pleasure and satisfaction.

THE LANCET.

London, Saturday, Feb. 14, 1835.

As we proceed in our consideration of what has been the conduct of the Almoners of *Christ's Hospital*, we feel at every step an increase of disgust. The whole case presents nothing but evidence of trickery or mismanagement. There is not a single point of relief, and the prospect before us is as dreary and unpromising as have been all the events and circumstances of by-gone errors. We inquire of the Almoners again,—Are they acting, with reference to the children in *Christ's Hospital*, precisely as they would conduct themselves with reference to their own children in their own nurseries? Whatever may be *their* answer to this question, we know full well what will be that of the profession and the public on the subject. Under the treatment of the medical officers of *St. Bartholomew's Hospital*, the disease "called *ring-worm*" has continued to rage unremittingly during the last twenty years, and during the last seven years of that period, Mr. Lloyd, one of those officers, in conjunction with Dr. Roberts, has had the control of the medical management of the children. Still no relief was afforded. In the month of May 1829, Mr. Lawrence, another officer of *St. Bartholomew's Hospital*, was consulted, in conjunction with Dr. Babington and Mr. Brodie. Thus the gentlemen, whose aid was sought at that time, consisted of the following individuals:—Dr. Roberts, Mr. Lawrence, Mr. Lloyd, Dr. Babington, and Mr. Brodie. Hence there was added to the two officers of *St. Bartholomew's Hospital*, to whom the treatment of the children was already entrusted, and who were utterly incompetent to cure the disease, Mr. Lawrence, *another* officer of that hospital, the colleague of Dr. Roberts and Mr. Lloyd; when, in order to secure an impartial report, in order to secure the possession of a document which should be effectually advantageous to the institution,—there were added the names of two other practitioners, and *only two*, thus giving to the officers of *St. Bartholomew's Hospital*, a clear majority over the individuals with whom they were called on to co-operate. This was an ingenious device, and of a piece with that sickening and cruel display of partiality which had been all along shown towards those medical men who

had proved, from the commencement of their appointment, that they were entirely ignorant of any mode which could be adopted for "eradicating" ring-worm, either from the town or the country establishment. Of course this Report was exactly what the Almoners had previously resolved that it should be,—a testimonial in favour of the measures which had been pursued by those officers whom they had injudiciously appointed, and whom they persisted in continuing in their institution.

The appointment, then, of the three officers of St. Bartholomew, in conjunction with two individuals of that establishment, was, to say the least of it, a manœuvre, of which the heads of a great public institution might, we should think, have justly been ashamed to be the authors. It was, however, to those persons who can see below the surface, an acknowledgment not only of wrong-doing, but also of their determination to persist in the commission of evil. Had they desired a strictly impartial and honest inquiry to be instituted, and, consequently, a perfectly just report, they would have sought for the assistance of men who were wholly unconnected with Mr. LLOYD and Dr. ROBERTS, and equally dissevered from every local interest attached to *St. Bartholomew's Hospital*; but instead of pursuing this straight-forward path of duty, they most unblushingly, under the pretext of instituting a full inquiry into the actual condition of the children, intrusted the examination and the framing of the report to a committee of five individuals, to their two incompetent officers, and to a colleague in another institution. Have we not a right to conclude, then, that their object was not to investigate, but *to conceal*? Have we not a right to infer that their chief motive was to obtain such a report as should have the effect of screening their own ill-judging management from the condemnation of afflicted parents and guardians, and an indignant public?

A majority in favour of their own officers having been secured, what said the investigators, the impartial inquirers, relative to the medical treatment, and the incinerated diet? We feel called upon to re-insert two paragraphs regarding these points, which we quoted last week:—

"We found the general health of the children in both establishments good; we felt convinced from their appearance that the diet is wholesome; we therefore do not think any change in it necessary; but there can be no objection, if it can be accomplished without inconvenience, to add a portion of vegetables to the dinner on the days when meat is allowed."

"We are of opinion that the various means which have been employed have been skilfully adapted to the changes and degrees of the disorder, and that, under present circumstances, no improvement can be suggested in this respect."

What could be more opportune for Mr. LLOYD, what more gratifying to the humane feelings of the Almoners, than this unequivocal testimony in favour of the system of therapeutics which had been pursued by their enlightened protegé? Of course Mr. LAWRENCE,—the surgeon who had been one of the *teachers* of Mr. LLOYD when Mr. LLOYD was a student, could not discover that it was possible to amend the plan of treatment which had been laid down by *his pupil* for the cure of the children! As for Dr. BABINGTON and Mr. BRODIE, we may put them aside as constituting an inefficient minority. The report was framed by Mr. LAWRENCE, and was necessarily carried by the majority of the inquirers, consisting of himself and his two colleagues.

What happens? Six years pass away, and every month, nay every week, of each of those years furnishes additional evidence that the medical officers of *St. Bartholomew's Hospital* are incapable of preventing the progress of the disease "called *ring-worm*," in *Christ's Hospital*, and in the

establishment at Hertford, or of curing the disease when it has actually occurred. The difficulty of the Almoners increased, and a blush of shame began to tinge their cheeks, when,—notwithstanding the able report of a Special Committee of Governors who were not Almoners, recommending that a gentleman of skill and experience, conversant with the diseases of the skin, should forthwith be appointed to reside on the establishment,—they fly for aid,—in breathless haste,—to what place? Why, *again to St. Bartholomew's Hospital*, and solicit as their chief hope *the interference of Mr. Lawrence*, the aid of that very man who six years before had told them that the system of treatment was PERFECT, and that nothing could be better for the health of the children than *an habitual diet of hog's-wash and hot cinders!* Now, the conclave of advisers is made to consist of Messrs. LAWRENCE, STANLEY, and LLOYD,—all three being bright geniuses of *St. Bartholomew's Hospital*, and Dr. CLEMENT HUE,—with whom we know not how to deal in this extraordinary affair, in consequence of the silence which is observed with respect to his name, he, be it remembered, being the physician of *Christ's Hospital*, and also a physician of *St. Bartholomew's Hospital*. It is, then, but fair to conclude that Dr. HUE is as incapable of curing ring-worm as are his colleagues, whose minds have been, or ought to be, strengthened by their far greater experience of the characteristics of that malady. What, we ask, can be more disgusting than the obdurate, the asinine persistence of the Almoners in consulting the medical and chirurgical officers of St. Bartholomew's Hospital? Had they resolved on perpetuating the ravages of ring-worm on the scalps and bodies of the suffering children, they would have acted wisely in entrusting the progress of the disease to WILLIAM LAWRENCE and his colleagues; but six years having elapsed since that gentleman alleged that the system of treatment adopted by one of his brother officers was PERFECT, is it not scandalous, is it not an audacious act of wanton cruelty in the first place, to neglect the recommendation of the Special Committee by introducing an incompetent person into the establishment; and, secondly, to persist in soliciting the very skilful aid of those officers who have already proved, by the sad experience of many years, that the ring-worm is an enemy against which they cannot adopt a single remedial measure with the slightest prospect of success? Yet this latter folly have they committed! Once more are the colleagues of Mr. LLOYD called forward, and desired to interpose their friendly aid. "Here, gentlemen," exclaim the Almoners, "pray come forward to "our assistance. We confide in you, be- "cause, like us, you must feel a deep "interest in vindicating the course of "medical treatment to which the chil- "dren in our establishment have been "subjected. To you, therefore, medical "officers of *St. Bartholomew's*, we hand a "*carte blanche*. Fill it up as you please; "direct as you may think proper. We "know that *our honour* is safe in your "hands, if the heads of the children are "not. Affectionately and confidingly we "leave you to your trust. As your multi- "farious occupations may prevent you "from seeing the children and treating "them personally, select a person who is "as worthy of your confidence as you are "of ours." The honourable surgeons accordingly make all possible haste to prove that they are entitled to the mark of favour which has been bestowed on them by the Almoners, and instantly proceed to select a champion who shall prosecute a victorious contest against that hitherto invincible foe, the ring-worm,—a champion far more mighty than they, and with whose strength and powerful intelligence

their own forces could bear no comparison. And who is this champion? Alas for misjudging human nature, these men of *St. Bartholomew* appoint a youth of *St. Bartholomew* for contending against an enemy by whom they have all been successively vanquished. It was surely a judicious and an honourable choice.

As we have already stated, we feel an increase of disgust at every step we take in reviewing these proceedings. Instead of appointing a surgeon of twenty years experience in the treatment of diseases of the skin, a young gentleman is chosen, who, up to the period of his appointment, does not appear to have considered that he was qualified to discharge the responsible duties of his profession. The name of this youth is BENNINGTON. We are most desirous of withholding any remark which may have the effect of proving injurious to him in his professional prospects. The fact, however, cannot be withheld, that Mr. BENNINGTON has been a pupil of Mr. LAWRENCE and Mr. LLOYD in *St. Bartholomew's Hospital.* Neither ought it to be concealed from view, that Mr. BENNINGTON paid the sum of fifty guineas for having been allowed to perform the subordinate duties of dresser in that institution. Mr. BENNINGTON, therefore, in being sent down to Hertford to be entrusted with the care of the children in that establishment, has, *by that act,* EMERGED *from his pupilage in St. Bartholomew's Hospital,*—from his pupilage under Messrs. LAWRENCE and LLOYD, under whose guidance he has endeavoured to acquire a knowledge of the practice of his profession, and of the principles on which the treatment of diseases should be founded. Amongst other matters he has, *of course,* been taught by Messrs. LAWRENCE and LLOYD, what they deem the best method of treating ring-worm, although it must be confessed that those gentlemen are far better capable of attesting, by the production of some hundreds of cases, those methods of treating the disease which had proved unsuccessful in their own experience. What, therefore, must be the surprise of Mr. BENNINGTON the PUPIL, the YOUTH, who has just been paying the officers of *St. Bartholomew's Hospital* for instructing him in his profession, on finding that he is *selected* by them,—by *his own instructors,*—to be entrusted with the management of a disease, the devastating progress of which, those officers themselves are *incapable of checking or controlling?* The usual course of events is here completely inverted. The student is called on to master a work which his preceptors acknowledge that they themselves cannot accomplish. Why, if Mr. LAWRENCE and Mr. LLOYD have made a proper choice, *they* ought to have been the pupils, and Mr. BENNINGTON the teacher. They should have paid Mr. BENNINGTON for instruction, and not he them.

There thus appears to be about an equal degree of culpability in the conduct of the Almoners and that of the medical officers. The impropriety of the choice made by the Almoners is about on a par with that made by the medical officers. The Almoners knew that the St. Bartholomew's system of treating ring-worm was totally inefficient. Yet up to this very hour they have persisted in applying to the medical officers of that hospital for aid. Those medical officers themselves know, from the experience of many years, that they cannot cure the ring-worm in *Christ's Hospital,* and yet they send to the Almoners one of those pupils to whom they have taught a system *which they know to be inert and worthless.*

Beyond this point folly cannot proceed one inch, and if knavery be at the bottom of the whole proceeding, nothing more knavish and unprincipled could have been devised and carried into execution. How long—to use a vulgar but an ex-

pressive phrase—will the great body of governors of *Christ's Hospital* permit themselves to be thus egregiously humbugged? Is no consideration due to the suffering children, or to the parents of the afflicted boys? Mr. BENNINGTON, *notwithstanding the testimony of* Mr. LAWRENCE *to the contrary before the Committee of Almoners*, although he is entrusted with the entire *medical* management of several hundred children of *Christ's Hospital* at Hertford, *is not even a licentiate of the Apothecaries' Hall*, as the following note addressed by Mr. WATSON, the secretary of the Apothecaries' Company, to one of the governors of *Christ's Hospital*, will sufficiently prove:—

"43, Berners-street, Jan. 30th, 1835.

"Mr. WATSON presents his respects to Mr. ———, and acquaints him that he has examined the lists of persons who have obtained from the Court of Examiners at Apothecaries' Hall, certificates of qualification to practise as apothecaries, and that he has not found the name of BENNINGTON among them."

In attending, therefore, to the medical management of the children, Mr. BENNINGTON, for every medicine which he prescribes and prepares, is liable, under the Act of 1815, to be mulcted in a penalty of twenty pounds, and, by the same statute, the Almoners themselves are rendered subject to penalties, by employing an *unlicensed* medical assistant.

But the Almoners may contend, that although they have been disappointed in what they expected, from the skill of the medical officers of their choice, yet that they have been fully justified in confiding in the *integrity* of those officers,—in their honour and disinterestedness. If it be really true that the Almoners are wholly deceived in this respect, it will become a duty on our part to show, on a future occasion, with regard to one of their favourites, that if his chirurgical skill in the treatment of ring-worm be bad, his utter want of principle in all things is a thousand times worse.

THE homœopathists, finding themselves making but slow progress in Paris, have had recourse to an expedient which, if it do not advance their doctrines, will at least bring them a moment's additional notice. At the meeting of the *Academy of Medicine*, on the 27th of January, the Minister of Public Instruction announced to the Academy that he had received a letter from the above-mentioned society, requesting authority to "establish a dispensary," and, when the necessary funds are collected, an "hospital" for the treatment of diseases on homœopathic principles. The minister, therefore, consulted the Academy to ascertain if it were proper to concede the request, whereon rather a stormy debate arose amongst the academicians, many of whom appeared to be frightened beyond measure at the very name of "homœopathy." One member having advised the bureau to enter into communication on the subject with the learned societies of Germany, M. MARC assured the Academy that lately, in conversation with a celebrated professor of Berlin, he asked him his opinion of the doctrine, and the answer was, "There are only three homœopathists at Berlin; one of them is a rogue, and the other two are ignorami." M. BRESCHET, in order to show into what disgrace the homœopathists had fallen in Germany, said, that he was lately present at a meeting of more than 600 German physicians, when one of them wished to bring forward the doctrine for discussion, but he was prevented doing so by a general cry of disapprobation. After some further debate in the Academy, a committee was appointed to answer the question addressed by the minister.

The founder of the doctrine, Dr. HAHNEMANN, was married the week before last, at the age of 84, to the young daughter of a Parisian proprietor.

Mémoires de l'Académie Royale de Médecine.
Tome Quatrieme, 2nd Fasciculus. 1835.
Published Feb. 6th.

We have just received the last fasciculus of these memoirs. They present some interesting observations, the most remarkable of which we proceed to analyze. The Number before us contains:—

1st. Remarks on the Structure of the Larynx and Trachea, by E. A. Lauth. Chef des Travaux Anatomiques at Strasbourg. 1 plate.

2nd. On the Obliteration of the Bronchi; by A. C. Reynaud. 5 plates.

3rd. Case of Imperforation of the Urethra and Anus; by J. N. Roux. 1 Plate.

4th. Case of Luxation of the Humerus, reduced, and perfectly cured; reduction after forty days; by A. Lepelletier.

5th. On a New Means of restoring the regularity of the Thorax in Cases of Lateral Deviation of the Vertebral Column; by M. Pravaz.

6th. On Rachitis, and the Alterations found in Patients who die of that Disease; by M. Salmade.

7th. Operations of Lithotrity practised with a brise-pierre à pression et à percussion. By M. Segalas.

Art. 1.—On the Structure of the Larynx, and the Trachea.

The object of M. Lauth in this memoir, which is strictly anatomical, and therefore does not admit of analysis, is rather to draw attention to some points in the anatomy of the larynx, either not before described, or controverted, than to give a complete work on the subject.

He commences by showing that the crico-arytenoid cartilages permit not only lateral movements, but also those of circumduction and rotation backwards; the cuneiform cartilages, which most authors place in the middle, between the arytenoid and the epiglottis, and which Bichat has altogether omitted, are situated quite close to the apex of the arytenoid cartilages, and are surrounded by a mass of mucous glands. In many cases, however, these cartilages are wanting; indeed the author thinks this disposition more frequent than their existence. The elastic fibrous tissue of the larynx serves the double purpose of causing motion, and also reinforcing the sounds produced in the larynx by its vibrations. It arises and spreads, in a radiating manner, from the angle of the thyroid cartilage, forming three principal fasciculi—viz. the middle crico-thyroid ligament, and the two cordæ vocales; the fibres, which extend upwards, form the superior ligaments of the larynx: thus all these parts, hitherto described by anatomists as distinct, are really so many portions of one continuous fibro-elastic tissue. The various dimensions of the cavity of the larynx, and the disposition of the glottis, are minutely described by M. Lauth, as also are the muscles of the larynx, amongst which he enumerates the glosso-epiglottic: this muscular band is placed immediately beneath the mucous membrane lining the base of the tongue. It commences by thin parallel fibres on the base, passes above the os hyoides and hyo-epiglottic ligament, and terminates in a tendon of fibro-elastic tissue, which is inserted, with that of the muscle of the opposite side, partly into the anterior surface of the epiglottis, and partly into the hyo-epiglottic ligament: this muscle is the antagonist of another (the ary-epiglottic), which is found in the membranous fold uniting the epiglottis to the summit of the arytenoid cartilage, and which extends from the arytenoid muscle, or a kind of fibrous raphe behind it, to be inserted into the edges of the epiglottis. The fibrous portion of the trachea, but seen at the posterior part, arises from the superior edge of the cricoid cartilage, and the base of the arytenoid, and extends downward somewhat like the longitudinal fibres of the arteries: the fibres of this tissue are so elastic, that M. Lauth has extended them to double their original dimensions, which they recovered. There are no elastic fibres connecting the edges of the rings of the trachea.

Art. 2.—Obliteration of the Bronchi.

This also is a production purely anatomical, a necessity to which the author was reduced, by the impossibility of finding, during life, symptoms sufficient to reveal the lesion of the lung of which he treats.

Before we examine the pathological conditions of any organ (says M. Reynaud), it is necessary to have a just idea of its normal structure. Haller and many anatomists of the present day are of opinion, that the minute ramifications of the bronchi lose themselves in a peculiar spongy tissue, whose

cells communicate in all directions, but MALPIGHI, and, in latter days, REISSEISSEN, have demonstrated most satisfactorily, that each bronchus goes on subdividing, without communicating with the others, and ends in a multitude of blind pouches (cul-de-sacs) which are all equally distinct. It is necessary to bear in mind this disposition, when we consider obliteration of the bronchi, of which the author describes four varieties.

The most simple is that which consists in the obliteration of the pulmonary vesicle, or cul-de-sac, terminating the bronchi. This species, to which various pathological lesions of the lung may be referred, exist not only in the obliteration of considerable masses, but also may be observed affecting one isolated vesicle, or a few together. However, the author prefers first treating of obliteration of the air-tubes themselves.

This alteration has been seen by M. REYNAUD, at all points of the bronchi, from the minutest ramification to the trunks; and he gives several examples of it, accompanied with drawings. In the lung of a patient who died of phthisis, he found at the surface several lobules, in which the bronchi, instead of terminating, as they generally do, perpendicularly to the pleuræ, passed obliquely, for the space of five or six lines, under the membrane, and then ended in their cul-de-sacs; the bronchus supplying one of these lobules was found upon examination to be obliterated not far from its termination; the air, which passed freely into its upper part, could not be forced on by the handle of the scalpel, and the tube manifestly degenerated into a little white solid chord, which, after having divided three or four times, terminated under the pleuræ, at the limits of the lobule.

A second species of obliteration the author has frequently met in the bronchi of the fifth or sixth order, distant a few lines from the surface of the lung.

In the third species, the obliteration was seated at the same distance from the pulmonary surface as the former, but whereas the tube in that case could only be laid open by the point of a fine scissars; here it admitted easily a blunt-pointed bistoury, which divided the bronchus until suddenly arrested.

The fourth species exists in the trunks of the larger tubes; the author has seen about twenty examples of this kind, in the third ramifications of the bronchi, and at variable points beyond that caliber, down to those which, in common dissections, are easily divided with the blunt-pointed scissars. When the bronchi were incised, which was done with the greatest facility, the tubes were seen suddenly to terminate in cul-de-sacs; but, externally, a solid fibrous chord, giving off various fibrous ramifications, might be traced from the cul-de-sac to the surface of the pleura. As to the seat of these bronchial obliterations, the author places them most frequently in the summit of the lung, the place most commonly attacked by other affections. The state of the obliterated tubes in that portion which remained accessible to the air was, in general, perfectly healthy; however, in some instances, they presented more or less appearance of dilatation, and in many cases the bronchi in the neighbourhood of the obliterated part were also considerably dilated. The bloodvessels of the affected part also seem to undergo a remarkable change; the larger arteries are healthy, and insufflation showed them to be pervious, as in a sound lung, but the smaller branches, which accompanied the fibrous filaments of the obliterated bronchi, refused all admittance to the air, and were, moreover, of a deep, dark colour.

The causes of obliteration of the bronchi are by no means easily ascertained; the author seems to incline to the hypothesis of inflammation, for the smaller tubes. In many cases, as has been remarked, the affection is accompanied by a deposit of tubercles, or dry lardaceous matter, in the summit of the lung, forming species of cysts or cavities, upon which the bronchi terminate by obliterating themselves. Perhaps here the obliteration is a consequence of interruption of function, in the same way as the ductus arteriosus contracts, when a more free passage is offered to the blood through the pulmonary artery.

In the second variety of obliteration described by M. REYNAUD, the bronchi are closed by a substance accumulated in the interior of the tubes; this only takes place in the small branches; sometimes the obliteration affected only one branch of the bronchial tree; in other cases all the branches constituting a lobule were filled.

The matter by which the bronchi are thus obliterated, is either tubercular deposit, or the product of inflammation, as seen in the false membrane of croup, &c.

In the first case the lesion of the lung is accompanied by the characters of phthisis; in the second it gives rise to a form of pneumonia and hepatization, which has not yet been described. The author details the case of a patient at *La Pitie* who died in a short time of a violent pneumonia; after death, on carefully examining the lung, the greater portion of the bronchi on the left side were found blocked with cylindrical masses of an ad-

ventitious matter, like coagulated lymph, or, rather, like the clots in an obliterated vessel; these extended down to the smallest ramifications, and gave the general appearance of a lung, affected with ordinary hepatization. Since the case just mentioned, M. Reynaud has seen six others; perhaps the affection is not so very rare, and now that the attention of physicians is directed to the point, more examples may be collected.

The last form of obliteration of the bronchi is produced by compression; of this, also, the author gives some examples, but the most remarkable is one derived from comparative pathology. In the year 1828, he had occasion to dissect the body of a monkey who died of phthisis; the lungs were filled with tubercles, and the left bronchus, close to its origin from the trachea, was enveloped by a mass of tubercular matter, exactly as the urethra is surrounded by the prostate. The pressure occasioned by this mass had so completely effaced the bronchus at its origin near the trachea, that on a longitudinal division being made, nothing remained of the cavity, except a line, or slit, about half an inch in length; below this the bronchi retained their normal caliber, and enclosed a reddish glairy mucus, without any mixture of air; the left side of the thorax was contracted to a remarkable degree, no doubt from the length of time which the lung had remained undistended by air.

Art. 3.—Imperforation of the Anus and Urethra.

Imperforation of the anus, in which no external sign indicates the neighbourhood of the rectum, is one of the most embarrassing cases that can present themselves to the surgeon, not only, as M. Roux observes, from the delicacy of the young patient and the gravity of the operation, but chiefly because the operator has no means of ascertaining the conformation of the gut at its lower extremity. The absence of an anal opening may, in fact, depend upon two conditions which are essentially different: in one the rectum has an abnormal opening into the bladder, or vagina; in the second case, to which alone the term imperforation properly belongs, the rectum either ends in a cul-de-sac, or the imperforation results from the presence of a membrane extending transversely across, or, finally, from the application together of its sides, which thus form a solid chord. It is clear, then, in the latter case, that an operation can be attended with no practical benefit, and that it is by no means easy to discriminate it from the former. On this principle, perhaps, we may account for the ill success which generally accompanies the operation for imperforate anus.

The subject of the case related by M. Roux was a child two days old, in whom the attendants had just recognised an imperforation of the anus. A naval surgeon was sent for, and perceiving the urethra also closed by a small membrane, he gave exit to the urine with a fine trocar, and called in M. Roux. The urine came away clear and in abundance. The child, however, continued to suffer from pain and vomiting. On examining the region of the perineum, no trace of swelling &c. could be perceived, nor was any fluctuation sensible to the touch.

Nevertheless, the author resolved on operating, and having placed the little infant as for the operation for the stone, he made an incision, ten lines in length, along the middle line of the perineum; the rathe did not exist. The edges of the wound being now held asunder, the fibres of the sphincter, whose internal edges were in close contact, were exposed to view, and the incisions prolonged through the levator ani towards the coccyx. The operator then found himself in a mass of cellular tissue, about an inch above the level of the perineum; he now laid aside the scalpel, and took a straight bistoury, whose point was directed upwards and backwards, so as to avoid the bladder. The instrument soon penetrated into a cavity, and on its being withdrawn, a quantity of meconium followed. A large quantity of mallow-water was then thrown up, in order to empty the bowels as quickly as possible, and the wound, which admitted readily the index finger, was dressed with charpie. The same dressing was continued for fifteen days, after which it was suspended, when the edges of the wound threw out a quantity of fleshy granulations which occasioned frequent colic, obstruction, and the following remarkable phenomenon. The stools, which had hitherto passed freely, became difficult, as soon as the artificial opening contracted, and the urine, instead of being healthy and clear, was now mixed with some excrementitious matter. The incision was at once enlarged towards the coccyx, and for some days the urine came away clear, but the granulations again sprang up, and the fæces passed off through the urethra. A large bougie was now placed in the artificial anus, and the granulations were touched with nitrate of silver; after this

the fæcal matter ceased to pass into the bladder, the cicatrix at the perineum became well formed, and the infant recovered all its natural functions.

M. Roux explains the passage of the fæces through the urethra, by supposing the existence of an abnormal canal, running from the cul-de-sac of the rectum to the urethra, in front of the prostate and bulb; indeed, he is so taken with this hypothesis, that he gives a plate representing the supposed disposition of parts in his patient, and the Academy have had the *bon-hommie* to publish it in the volume before us.

This is the first time that we have seen the representation of a complicated anomaly given while the patient was alive and doing well; as to the probability of its being a resemblance, we cannot agree with the author. The case, according to our idea, would be one, not of imperforation of the anus, but of anomalous opening of the rectum; we conceive that the rectum terminated by a small orifice in the posterior wall of the bladder, its neck, or even perhaps of the urethra; as to the abnormal canal of M. Roux, formed by the deviation of the inferior hæmorrhoidal artery, we believe that nothing analogous has as yet been seen in nature.

Art. 7.—Lithotricy, performed with a brise-pierre à pression et à percussion.

We have, in a former number, spoken of Dr. Segala's instrument, for which, the academy of sciences rewarded the author with a prize of 2000 francs. In its mode of action, it unites the brise-pierre of Mr. Jacobson, with the curved percuteur of Mr. Costello, in so complete a manner, that the pressure and percussion succeed one another instantaneously. The present paper contains a series of seven cases, in which the instrument was employed with remarkable advantage: according to the author, neither the volume of the stone, a catarrhal or a paralytic state of the bladder, engorgement of the prostate, nor excessive curvature of the urethra, has prevented it from acting with efficacy.

The facts presented by M. Segalas may be distinguished into three orders; the first comprises cases in which the stone was small, and destroyed at one sitting; the second, stones of moderate size, requiring two or more operations; the third, cases in which the calculi were large and hard, and the disease, moreover, complicated with catarrh or paralysis of the bladder, enlarged prostate, &c. We shall endeavour to give a rapid analysis of each:—

Case 1.—The stone was small, the child 12 years old. He had had difficulty of urinating for some time, and just before the operation a complete suppression occurred. The calculus was destroyed by simple pressure. In two hours the bladder was completely freed from all detritus, and the child returned home in the evening perfectly cured.

Case 2.—This patient was 70 years of age. The stone appeared to be small, and the urethra was free. Some warm water having been thrown up into the bladder, the brise-pierre was introduced, and the calculus seized, without the patient's being conscious of what was passing. Part of the stone, which was composed of uric acid, came away with the instrument, and the rest in a few days. No fever succeeded, and the patient did so well, that he gave a dinner to his friends next day, and did the honours of the table. This was followed by an agitated night, and some fever, with congestion to the head, but these were soon removed by appropriate means.

Case 3.—The patient, 61 years of age, had had symptoms of stone for six months. The urethra and bladder were very sensible, and the prostate was much engorged. At first it was difficult to seize the stone, but in raising the pelvis, it fell between the branches, and was immediately crushed. The diameter was nine lines. Some fragments came away on the day of operation. No fever ensued; three days afterwards the operation was repeated, and since then no fragments have been to be felt.

In this case the straight instrument would, in all probability, have failed.

Case 4.—The patient was 70 years of age, and had been several years affected. On the 6th of January he was attacked in the street with an apoplectic fit, which terminated in hemiplegia. This was dissipated by bleeding, &c.; and in forty-eight hours afterwards the operation of lithotricy was performed. The stone was ten lines in diameter; pressure alone destroyed it. The fragments were discharged; no fever ensued. The operation was renewed on the 11th and 15th, but this last time no fragment was found.

The urine has since then been clear, and free from all trace of blood.

Case 5.—The patient was 64 years of age, and had been affected with gravel for fourteen years, and, latterly, had had symptoms of stone. Immediately before the operation he was attacked with complete retention of urine. On the introduction of the sound, the calculus was found in the neck of the bladder. It was pushed back, and seized, after a little slipping, with the branches. The stone was round, and eleven lines in diameter; pressure was sufficient to break it. Some fragments came away on the day of the operation. No accident followed. Lithotricy was repeated four days afterwards, and a third sitting took place in seven days. This completed the destruction of all the fragments. Since then the urine has been clear and free, and the patient now enjoys perfect health.

Case 6.—The patient was 46 years of age. The symptoms had existed for several months, with bloody urine, &c. On sounding, spasmodic stricture of the urethra occurred, and irritable bladder. The urethra was accustomed to bear wax sounds for some time, and then the operation was commenced. The stone was thirteen lines in diameter, and easily divided by pressure; however, as the instrument was much constricted by the irritable bladder, the operation was not pushed further. Lithotricy was repeated four days afterwards; a good deal of detritus came away and no blood. After the lapse of four days, a third sitting took place, and since then all bad symptoms have disappeared, and the patient is now perfectly well.

Case 7.—The patient was forty-five years of age, and was attacked thirteen years ago by violent symptoms of stone suffering excessive pain in urinating, with bloody discharge, and having great difficulty of walking. He was recommended to take the waters of Contrexeville, and after using them for some months he passed two fragments of a calculus, of the shape of the quarter of an orange, and of a size indicating that the whole stone was 16 or 18 lines in diameter. Other smaller fragments were successively passed, and the patient seemed cured. Symptoms of stone, however, returned, after some years, and persisted, in spite of a similar treatment. Having come to Paris, he took a great deal of exercise, and was attacked with acute inflammation of the bladder; this was removed, and the patient was then sounded. The bladder was still very sensible; the patient urinated every ten minutes, and with pain. Three days afterwards, as he was urgent, the instrument was introduced, and the stone was seized; it proved to be nineteen lines in diameter, and was broken down by mere pressure, some of the fragments coming away. The operation, instead of irritating, calmed the state of the bladder. Lithotricy was repeated two weeks afterwards, no accident ensuing again. It was repeated on the 7th, 17th, 23rd, and 29th, the irritation of the bladder gradually disappearing, and since the last sitting all symptoms of stone have been removed. Several practical remarks may be drawn from this case. In the first place it proves that it is possible to commence lithotricy even when the inflammation of the bladder has not been quite subdued; and, moreover, that the operation, instead of aggravating the disorders, may calm the symptoms; finally, it shows that the employment of internal remedies is not always without benefit.

Case 8.—The patient was seventy-five years old; he had passed gravel for some months, and presented symptoms of catarrh of the bladder. On being sounded, a stone was recognised, and a quantity of stinking puriform urine came away. After the lapse of a few days from the first sounding, the brise-pierre was introduced. The stone was 21 lines in diameter, was easily divided, and then the fragments were attacked. Some detritus came away, but the urine was bloody, and this much alarmed the patient. Emollient injections were now thrown up, and brought away more fragments. The sittings were repeated from time to time, and after the sixth the cure was complete.

Case 10.—The patient was fifty-five years of age. The first symptoms occurred twenty-three years ago. The symptoms were at first combated with success by vegetable diet and the waters of Contrexeville; but relapse came on, and the disease was now attacked with bicarbonate of soda, as the gravel passed was clearly uric acid. This gave great relief. The symptoms returned again in four years, and the patient was sounded, when the presence of a stone was ascertained. The urethra was contracted at a few inches from the orifice. This was combated by dilatation and caustic, and then the instrument was passed into the bladder, but it was impossible to seize the stone. The surgeon again tried in a week, with the same result. Finally, after a lapse of some time, a moderate-sized brise-pierre was introduced, and the stone was seized; it was 22 lines in diameter; it gave way readily, and fragments came away. In eight other sittings the calculus was completely reduced, and the patient's health was restored.

In the last case, reported by M. Segalas, the stone was excessively hard, and presented a diameter of twenty-three lines; in addition to this the patient, an old man of seventy years of age, was affected with stricture of the urethra and enlarged prostate. The first attempt with the brise-pierre was unsuccessful and the author was on the point of having recourse to the high operation, when he had a larger and stronger instrument made, with which he succeeded in catching and reducing the stone, after twelve sittings.

Dissertation on the State of Medical Science, from the Termination of the Eighteenth Century to the present time. By W. Alison, M.D., F.R.S.E. (From the "Cyclopædia of Practical Medicine." London, 1834.)

Medical science, since the close of the last century, has been cultivated with unexampled ardour, and has adequately progressed during the period which has thus elapsed. Many circumstances have greatly favoured this gratifying advancement, though medicine has at the same time had to struggle with difficulties which ought not to have impeded so important a branch of knowledge at any instant of its history, especially in this latter portion of its career. Once, however, blessed with a perfect freedom of constitution in the governing medical institutions of all countries, and especially in our own,—abundantly supplied as the art is with enthusiastic followers, yet far more splendid results may be promised as the reward of its cultivation in another third of a century than we have witnessed even in the past third, some account of which we have here before us in an essay by Dr. Alison of Edinburgh.

This is the first attempt of the kind, on the same scale, which we have yet seen, though it seems strange that the varied products of so many researches in medicine should have lain dispersed so long over the field of inquiry, without exciting one successful attempt to collect the fragments together, and to raise the whole into a system. Perhaps, however, as Andral and several other eminent pathologists believe, it is impossible to co-ordinate all our present knowledge, to bring "bone to his bone," and to breathe life and durability into a comprehensive medical theory; indeed, past experience and the course of the more exact sciences seem to render this difficulty unquestionable, and to point out the more useful expedient of examining in distinct groups the history, and the real value, of the doctrines which are now current. To this latter task, Dr. Alison has addressed himself, and has noticed nearly all the important truths which have derived their origin, or gained a firm footing amongst us, since the days of Cullen or of Hunter.

The present century derived its medical character from the close of the last,—represented in England by these two distinguished men; but Reil in Germany, and Bichat in France, exerted an influence equally powerful over their successors, who, it must be confessed, have been more industrious, and, in many instances, more fortunate, than ourselves, in proportion, perhaps, to the superior stimulus which was supplied to them by the scientific institutions of their respective countries.

Dr. Alison has, in general, done strict justice to the French writers, and has noticed their productions with candour; but on the part of the Germans there is cause to complain that he either has not known, or has not duly appreciated, several of the late researches and acquisitions made in their schools. This is the more to be regretted in a dissertation on the present state of medical science, as, since the Germans ceased to write in Latin or French, their discoveries and opinions have, with the exception of a few exceedingly extravagant views, been slow in making their way before the English public; although the appearance of numerous late works entitles us to deny the applicability of Walther's quotation, "Et penitus toto divisos orbe Britannos."

In the first section of his work, Dr. Alison discusses the modifications induced in Cullen's theory of the primary moving powers, and spasm—explodes explanations founded on final causes, or the *vis naturæ medicatrix*,—examines the causes of *sudden* death, now attributed to a

stoppage of the flow of arterial blood throughout the body,—reviews the discoveries that the different offices to which the nerves minister, depend on differences in their endowments,—and enumerates the important additions made to our knowledge of the blood, of muscular contraction, of the structure of the heart, of the causes of the venous circulation of the capillary circulation, and of the functions of nutrition, secretion, and excretion.

Of the various discoveries which have been made regarding the intimate structure of organized bodies, the most important are:—

"The very general tendency of substances which are the result of vital action, to take the form of globules, or rather of cells, in which a containing cyst, and a contained matter, are usually discernible; the total absence of crystalline arrangement in the living and growing parts of these textures; and the fact, that the particles of earthy and saline matters which enter into the composition of organized substances, however small their proportion to the whole, are never aggregated together, but are equally diffused through the whole mass, and retain their original form and dimensions even after the whole of the strictly animal or vegetable matter has been burnt out."

It is nearly established that "the different *excretions* already exist in the "compound blood, and are only evolved, "or separated, not formed from the blood, "at those organs at which they respectively appear. This is certain as to the "urine."

It may correct some errors still prevailing, to observe,—

"That although the set of vessels described for a century past by the name of absorbents are really destined to the office of absorption, and their structure, in various ways, is peculiarly adapted to it, yet it is not through them, nor through any one set of vessels exclusively, that the absorption of extra-vascular substances into the circulating mass is effected; that a function precisely similar is executed in many living beings, without any set of vessels being appropriated to it, and that the absorption of extra-vascular matters in the higher animals must be ascribed, therefore, in the last result, to peculiar relations subsisting in the living state between those matters and the circulating blood, rather than to the peculiar nature or forms of the vessels which are the organs of absorption; and, accordingly, that the degree of absorption is very much influenced by two circumstances in the condition of the circulation at the part where any such extraneous matters may lie—viz. by the fulness of the vessels, and by the rapidity of the current at that part; being always diminished when the vessels are much distended, and likewise when the flow along these vessels is much retarded or suspended, as by the removal of atmospheric pressure from any portion of the surface of the body."

"The peculiarities of the blood of the fœtus have also been examined; and the curious discoveries of Sir Astley Cooper and Dr. Lee of the formation of albuminous matter in the thymus gland and in the liver of the fœtus, have distinctly shown that here, as well as in the adult, there are contrivances adapted to the general object of assimilation, the particular intention of which is still mysterious."

The exclusive "solidism," so long taught to all her graduates by the Edinburgh schools, is fairly renounced as untenable:

"As we must take the fluids, and the changes proper to the fluids, into account in all attempts at explanation of physiological phenomena, so we must be prepared to admit a *humoral pathology* as essential to the explanation of all the more important phenomena of disease; but this must be a pathology founded on observed changes, not simply of the mechanical or chemical condition, but of the strictly *vital properties* of the fluids, and especially of the blood."

Among the topics brought forward in the second chapter, are the influence of cold, impure air, and hunger, in predisposing to disease; the contagious nature of epidemics, and the laws of their propagation; the distinction now drawn between the different textures and parts of an organ inflamed, as instanced in the pleuræ, the bronchiæ, and the parenchyma of the lungs; the inflammation of the membranes of the brain, distinguished by its intensity, and the frequency of softening in that organ; the coincidence of a granulated state of the kidney, and an albuminous state of the urine; the occurrence of tubercle without inflammation; and the *specific* varieties of inflammation, erysipelatous, syphilitic, gouty, and rheumatic.

In the following passage, Dr. Alison expresses an opinion, which is at variance with the general voice; but his judgment

is founded on extensive experience; and so long as the value of medicine is not rigorously investigated, by comparing, counting, and stating *all* the necessary facts will deserve attention.

"It has often been represented, that not only the purgative mercurial medicines, as general evacuants, and as means of acting particularly on the secretion of the liver, are of peculiar importance, but the action of mercury on the system at large is the surest means of controuling those effusions, and particularly the effusion of coagulable lymph, on which the danger of several inflammatory diseases essentially depends; and in some instances, a less defined and more specific virtue in checking inflammation has been attributed to the constitutional affection from mercury. Thus the remedy has been successively vaunted in the case of acute *hydrocephalus*, of *cynanche trachealis* or *laryngea*, and (generally in the form of calomel and opium) in that of *pneumonia*, *bronchitis*, *pleuritis*, *pericarditis*, *peritonitis*, *hepatitis*, and *dysentery*; and reference has been often made to the effect which may be observed from it in inflammation of the iris, tending to effusion of lymph there, as demonstrative evidence of its peculiar efficacy. But it may be stated with confidence, that in the opinion of many of the best informed members of the profession there has been much exaggeration in all these statements.

"It has been clearly shown, by Dr. Armstrong and others, that when full bleeding has been premised, opium may be given freely and repeatedly, often with extremely good effect as to the relief of suffering, certainly without any injurious effect on the alvine evacuations (which seem frequently to be promoted or procured more easily by enemata after the use of opiates), and, it may be pretty confidently asserted, with a considerable diminution of the mortality below what is common when blood-letting and purgatives are the only remedies employed. In the cases which the present writer has observed, the beneficial effect of opium has been best marked when the pulse has been early much depressed, and has not risen after blood-letting, so that farther loss of blood has seemed hazardous or impracticable."

The task of balancing the claims of rival individuals, of dealing with modern doctrines, and of accumulating facts which were scattered through innumerable volumes, has been sufficiently arduous, and has, for the most part, been accomplished by Dr. Alison with success, good judgment, and impartiality. We shall not discuss here all that appear to us to be faults in the dissertation; but it will be our duty to notice a few of the omissions, and we shall offer some objections to a doctrine which is sanctioned by the author, and held by that decreasing remnant of physiologists who deny that the ordinary laws of nature can be appealed to in explanation of the functions of organized bodies.

Magendie, although not the first, has been one of the most constant and powerful opposers of the exclusive system, and on every occasion he has most successfully exposed its imbecility. The multiplied discoveries set forth in the lectures of that professor which we have lately published, are so many triumphs of the rational system, which teaches us to compare the phenomena that occur in animated bodies, with each other, and with similar phenomena in the air, in the ocean, in the earth,—in all the masses of matter from which animals are formed, and into which their substance ultimately again resolves. The mechanism of locomotion; the circulation of the blood through tubes; the exhalation of aqueous vapour from the skin and lungs; the formation of carbonic acid and of heat, which is dissipated, when excessive, by evaporation; the penetration of membranes by gases and liquids,—could never have been understood apart from the "study of dead matter," and should constantly lead us to suspect that if "the applications of natural science to medicine are very partial," the fault really lies in our ignorance of chemical and physical, as well as of vital processes. Dr. Alison says, "Although some degree of acquaintance with other natural sciences is properly expected of a physician, yet it is chiefly as an exercise of the understanding, that the study of these sciences must be recommended." If it was meant to affirm that many vital conditions can be studied, and to a certain extent explained, without the possession of a knowledge of the sciences, or that close observation of the living body itself is essential to an acquaintance with its functions, investigation of no other kind being capable of superseding that study, the statement embodies a palpable truth, attention to

which would have preserved the learned men of the last century from committing innumerable mistakes, and repeatedly misapplying their principles; but the two pages in which this notion is developed, obviously prove the design of enforcing the more dangerous and untenable opinion, — that an intimate acquaintance with natural philosophy and chemistry is by no means indispensable to the physician. It is hardly necessary, at this time of day, for us to point out that some of the first principles of physiology can only be explained by the laws of mechanics; that in practice, chemistry admits of almost as frequent application as anatomy; that the material elements in assuming organic forms do not lose their essential nature, whose modifications probably constitute all their new properties: and that our limited knowledge of physiology, morbid conditions, and the action of remedies, does not entitle us to disdain any one of the aids which are to be derived from natural science. On the septuagenarian professor or practitioner it were useless to urge the utility of studies which would now be to him not only painful but impossible; they may be dispensed with among the few who can rely for a knowledge of disease on the possession of much collegiate morality, and considerable stores of Latin and Greek, but the young men of science who must soon form the bulk of those regenerated institutions which will take the place of ancient and imbecile medical corporations, cannot for their future fame afford to listen to dicta which accord so well with the habits of natural indolence.

"Vitanda est improba Siren
Desidia."

The application of the instruments and principles of natural science to the investigation of living matter, promises to be the richest mine of future discovery; for the probability, *a priori*, is, as MECKEL before his death prophetically announced, that the forces which actuate the organism will be found to govern the universe. If the tree be known by its fruits, it may be curious to inquire what has been done for science by the exclusive vitalists, who treat as sacrilege all meddling with the principle of life? Turn over the monotonous pages of ADELON, the eloquent but unscientific work of RICHERAND, whose long heresy is scarcely expiated by his late recantation, or glance for a moment at the uncertain experiments and inconclusive reasonings of WILSON PHILIP, *et hoc genus omne*, and then examine the labours of PROUT, BOSTOCK, THOMSON, ALLEN, and PEPYS, of MAGENDIE, BLAINVILLE, CUVIER, and RASPAIL; of BERZELIUS, TREVIRANUS, TIEDEMANN, GMELIN, and the German naturalists,—inspired by the spirit of exact science, and rich in substantial contributions to medicine,—a cloud of witnesses sufficient to convince dispassionate observers that no permanent advancement can be made in physiology, excepting by those who are thoroughly grounded in the principles of general science. It is an incontrovertible fact, that all who have latterly made solid additions to our practical knowledge of disease* are well versed in the natural sciences. Indeed the great physicians of every age have been familiar with the physical knowledge of their time.

The phrenologists are attended to, but GALL and SPURZHEIM are never mentioned in this dissertation. Whatever opinions may be held with regard to the certainty of their discoveries, the historian who could pass unnoticed the authors of the "Anatomie et Physiologie du Système Nerveux," must be strangely blinded by local associations, prejudice, or want of philosophic discrimination. Experiment, and the facts of morbid anatomy, scarcely justify the confidence with which the functions of the parts at the base of the brain are specified; or the mention which is made of some of the experimenters The tame and uninspired speculations of the Scotch metaphysicians, honourably distinguished by the author, are now never read; the thread of their fate has long since been cut asunder; and they (we except SMITH and BROWN) ought now only to be found in a metaphysical obituary.

Among the omissions which most detract from the value of Dr. ALISON's work, are a clear account of late discoveries re-

* This is illustrated by various articles in the "Cyclopedia."

garding climate, and the philosophical precision which has been given to the employment of hygienic means as therapeutic agents, by Dr. CLARK; Dr. ELLIOTSON's interesting researches into the properties of medicines; LOUIS's rigorous application of figures (*methode numerique*) to pathological facts, usually generalized by guessing instead of counting; the extended views, new facts, and eloquent exposition of comparative anatomy, by Dr. GRANT; KIERNAN's discoveries in the liver; and Dr. HODGKIN's industrious inquiries in pathological anatomy; some not unimportant discoveries in the blood. Dr. CARSWELL's splendid collection of drawings at the London University, with his writings, promises to place us on the eminence we had attained in this department by the works of BAILLIE.*

At another time we shall perhaps notice these, and the labours of some other distinguished men, who appear to have been forgotten. Dwelling on what certainly are imperfections in Dr. ALISON's dissertation, it is just to say that there are parts of it which will be read with great pleasure. Few men of his class would have written contemporary history with more fairness, have exhibited prejudice on so few subjects, or have brought together so much general information in so useful a form. With the article of Dr. BOSTOCK on the "Ancient History of Medicine," it terminates the "Cyclopædia of Practical Medicine,"† of which we here take the opportunity of saying, that it exhibits in many of its articles, not only a full digest of all the facts before known, but many important additions to medical science.

* Something stronger than this we recollect to have fallen from Andral, in the introduction to his course of medicine (1834).

† The Bibliography by Dr. Forbes is yet to appear.

TREATMENT OF STONE.

To the Editor of THE LANCET.

SIR,—I shall feel much obliged if you can inform me where the Hospital for the Treatment of Stone in the Bladder, mentioned in the last number of your publication, is situate, as poor people labouring under the disease have occasionally applied to me for relief. I agree with you that stone in the bladder is by no means a pleasant or an agreeable companion. The *horrors* and *dangers* of the operation by the knife have been occasionally, in your invaluable publication, much magnified and overrated. It is the fashion to decry this proceeding, and it is done to serve a very bad purpose. The average of success is not correctly given in the document referred to. In the hands of a good lithotomist,—of one who has been at the trouble to acquire due dexterity, without which none should be permitted to make the attempt,—not more than a minute, often much less, is spent in the operation, and the deaths (taking all proper cases of every age that present) are not above one in fifteen or twenty. When the stone is very large, the danger attending its removal is certainly much increased, but in such cases *no other proceeding* can avail in bringing about a cure. It is not to be denied that some very horrible and bungling cutting operations have been performed, and that unjustifiable force has been resorted to; and some most cruel and scandalous operations, worse than even the others, are well known to have occurred in the hands of the best of the lithotriters. The patient, it is said, is bound hand and foot. He is not a whit more confined or bound than in lithotrity. Humanity, it is said, demands the concealment of the results of lithotomy. These have never been concealed, but the same cannot be said of the new operation of lithotrity. False facts prevail in the statements of those gentlemen who practise and patronise it. What safety is to be expected for patients in an hospital such as the one proposed? Civiale and Heurteloup have tried cutting; the latter, it is said, lost the only two on whom he tried his hand, and the former, from his constant bungling and want of success, has been prohibited from cutting in the hospital which was established for him. There never can be safety for patients labouring under stone, *in any class of society*, till the operation is practised by well-educated surgeons, and taken out of the hands of the charlatans who now usurp the ground.

There is no mystery in the matter, no difficulty which cannot be overcome by those who will be at the pains of studying and practising the proceeding. It has been resorted to with perfect success in some of the hospitals of this city, in St. Bartholomew's, in Dublin, in Glasgow, and in many of the provincial towns. In the Edinburgh Hospital, lithotrity has, for a series of years, been employed, *in all cases suited for it*, and with almost invariable success. The public ought not to be called on to support any hospital for this operation exclusively. If patients apply for relief to a man, who can *crush* or *grind* stones only, they will be subjected to that proceeding, whatever the circumstances of the cases may be. If to a man who can only *cut*, they will all be cut. Such is my view of the matter at least, and of many with whom I have conversed on the subject. I am, Sir, yours, &c.

ANTI-HUMBUG.

Hammersmith, Feb. 9, 1835.

CYCLOPÆDIA OF PRACTICAL SURGERY.—*To the Editor.*—Sir, in reply to the inquiry respecting the "Cyclopædia of Practical Surgery," contained in the notice to correspondents in your LANCET of last week, I beg to refer you to the advertisements of that publication, which I understand will appear in the periodicals of the present week. I have the honour to be, Sir, your most obedient servant,

WILLIAM BLOXAM.

26, George Street, Hanover Square,
February 10th, 1835.

WATER TRUSS PADS.—*To the Editor.*—Sir, In a late number of THE LANCET a correspondent inquired where "water truss-pads" were to be obtained. I beg to state that the inventor (Mr Forster) has now brought them to a state of great perfection. The superiority of the water-pad to any hitherto manufactured will ultimately lead to its universal adoption, and to the expulsion of those now used. Cases of hernia have occurred in my practice which have failed to derive the necessary support from any of the common pads, but which were efficiently restrained by a truss having a water-pad, its elastic power being completely under the control of the wearer, the quantity of water being regulated by a valvular opening. Another invention of Mr. Forster is a bag of thin sheet Indian-rubber, to be used in diseases of the testicles which require the constant application of cold; a wet sponge or linen can thus be applied while the patient pursues his ordinary avocations. As the manufacturer has not at present a convenient place in which the profession could inspect the articles, specimens of them may be seen at my residence by any professional gentleman before twelve o'clock, and the address of Mr. Forster obtained. I am, Sir, your most obedient servant,

G. R. MART, Surgeon.

19, Frith-street, Soho-square,
January 26, 1835.

CORRESPONDENTS.

The paragraph signed *A Constant Reader* should have been authenticated; besides, we are at a loss to know what is the nature of a "medical assistantship to a dispensary."

As *Mr. A.* regards the objections to the antiphlogistic plan as "too erroneous to deceive any thinker in the profession," we have less hesitation in laying his communication aside. As a means of caution to non-professional readers, it is too technical.

A notice of the death of Dupuytren will be found at page 702 of our present No.

The letter of a *Country Practitioner*, on "Ophthalmic Quackery," forwarded to the Council of the College of Surgeons, is replete with excellent points, but, unfortunately, the whole document is libellous.

J. B. A sea voyage might completely re-establish the health of the child, and a cold climate, if it be dry, is not injurious to the disease named by our correspondent.

A Pupil at the Webb-Street School complains that the diagrams used by Mr. Everett are not sufficiently clear to convey information.

a. We will see what can be done. This week it is not possible. Thanks for the expression of good feeling entertained by the writer.

A. The pamphlet and letter reached us. The animal is too good to be allowed to escape, and shall be safely netted.

We have in type the letters of A Blue—Mr. Richards—A Guy's Student—Vena Portæ—Mr. Mole—Dr. Davidson, and Dr. Loudon, for all of which room shall be found next week.

BOOKS RECEIVED.

Account of the Epidemic Cholera as it appeared in Cincinnati. By Daniel Drake, M.D. Ohio, 1833.

The Medical Pocket Book for 1835, containing a Case-book and Almanack, with Accounts of Medical Corporations, Societies, and Schools; with Information relative to Drawing up Cases, Medical Evidence, Poisons, New Medicines, and an Abstract of the Pharmacopœia. By John Foote, jun. London. Renshaw, pp. 166.

The Dublin Journal of Medical and Chemical Science; including the latest Discoveries in Medicine, Surgery, Chemistry, and the Collateral Sciences, to Jan. 1835. Dublin. Hodges and Smith.